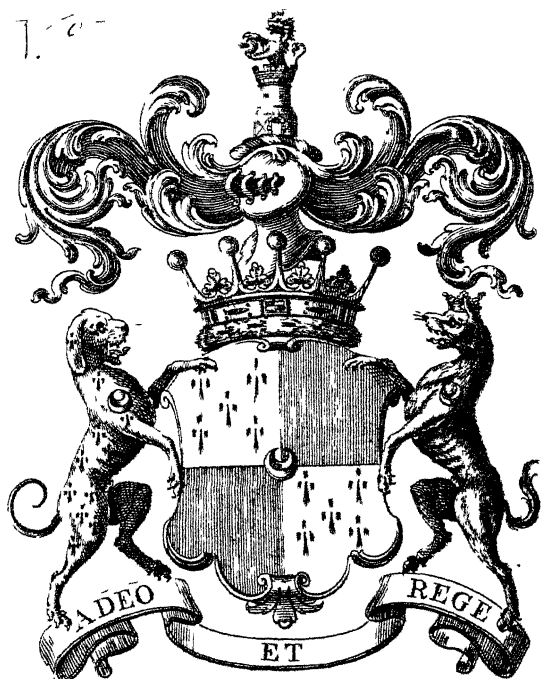


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Philip Carl Stanhope.



AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL
TRANSACTIONS,

GIVING SOME

A C C O U N T

O F T H E

Present Undertakings, Studies, *and* Labours,

O F T H E

I N G E N I O U S,

I N M A N Y

Confiderable Parts of the WORLD.

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- P. 96. in the Title to Art. XVII. for 1757, read 1758.
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PHILOSOPHICAL TRANSACTIONS.

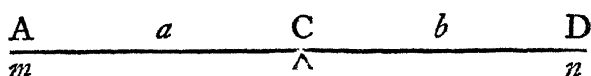
- I. *The greatest Effect of Engines with uniformly accelerated Motions considered.* By Francis Blake, Esq; F. R. S. *

Read June 24, 1756. **T**HE writers, I have met with, upon the *maximum* of Engines, or the greatest effect possible in any given time, have supposed the working parts of the machine to retain their direction, and be uniformly moved by the force of a current. They have therefore considered only the case of an uniform Rotation, as in the action of grinding; where the impediments and impulses being brought to a balance, the impulses are but sufficient to prevent a decay in the generated motion. And, upon that view of the Problem, the load of an Engine, when

* The distance of time betwixt the reading of this paper, and its now appearing in the *Transactions*, was owing to the absence of the author, and his desire to reconsider it before it went to the press.

the effect is a maximum, and the force a current, is determined by computation to be *four ninths* of the weight which would cause the Engine to rest. This, then, being suited only to an uniform velocity both in the lever and obstacle, I would consider the case of an uniformly accelerated one in repeated vibrations. The maximum which corresponds to it is adapted to the steam-engine, and of no less importance to be determined than the other. And, no doubt, these speculative inquiries into mechanical subjects, were they even barely considered as our guides in experiment, may appear to deserve the attention of the Society. But indeed, and I ingenuously confess it, an opportunity to make some remarks upon what I formerly laid before you, concerning the proportion of the cylinders, was a further inducement to the present research.

A general expression for the time of a stroke in such vibratory Engines, will lead us without trouble to a computation of their effects.



Let AD be a lever, whose Brachia are a and b , and supposed without weight. Let m be a Power, and n a weight. Then $a : b :: n : \frac{bn}{a}$, the balance for n at A, and $m - \frac{bn}{a}$ is the effective force at A, which multiplied by the lever a gives $ma - nb$ for the efficaciousness of that force in the angular velocity of the Power and weight. Now, by the principles of Mechanics, the Inertia of any bodies revolving

volving about a Center is as the quantities of matter into the squares of the Brachia; and in the present case, therefore, the whole Inertia of m and n is as $ma^2 + nb^2$. Hence then, and because the velocity generated in a given particle of time is as the Force directly and Inertia inversely, we have $\frac{ma - nb}{ma^2 + nb^2}$ as the accelerating force, or the measure of the angular velocity of the Power and weight at the end of the said given particle of time. And I use the angular velocity, because the arbitrary proportions in the lengths of the Brachia which may form an Equilibrium will not alter the expression. But again, the times of descent by means of uniform forces, thro' a given space, are inversely as the square roots of the accelerating forces, or measures of the velocities generated in a given particle of time; and therefore

$\sqrt{\frac{ma^2 + nb^2}{ma - nb}}$ is a general expression for the time of a

stroke. This being had, the solution is easy; for,

supposing n only to be variable, say as $\sqrt{\frac{ma^2 + nb^2}{ma - nb}}$

: $n :: 1$, a constant or given time: $n \sqrt{\frac{ma - nb}{ma^2 + nb^2}}$

the effect in time 1, *ex hypoth.* the greatest effect which can possibly be produced in the said given time. Taking, then, as usual, the Fluxion equal 0, we have, after a proper reduction, $2a^3 m^2 - 3a^2 mnb$

$+ amnb^2 - 2n^2b^3 = 0$, and $n = \frac{am}{b} \sqrt{\frac{a}{b} + \frac{3a-b}{4b}}$

$-\frac{am \times 3a - b}{4b^2}$. Therefore, in these sorts of En-

gines, when the Brachia are given, the weight :
B 2
Power

Power $\therefore \frac{a}{b} \sqrt{\frac{a}{b} + \frac{3a-b}{4b}} - \frac{a \times 3a-b}{4b^2} : 1$; and if the Brachia are equal, *i. e.* if $a = b$, the weight : power $\therefore \sqrt{\frac{1}{4}} - \frac{1}{2} : 1$, *viz.* 0,618 : 1 nearly when the effect is a maximum. And so, in like manner, when b , m and n are given, and a is made variable, it is easy to see that, instead of the load, the best distance of the power from the fulcrum of the lever will be the result of the process; *viz.* $a : b :: n + \sqrt{n^2 + mn} : m$. But, this by the way.

In the proportion here determined, the power m is a weight, and therefore $ma - nb$, which is the generating force, being partly employed to overcome the Inertia of the quantity of matter m , it is not wholly taken up in giving motion to the weight n ; and the relative velocity is continually decreasing. But, on the other hand, if m be the force of a spring, as is that of our atmosphere, or if n can be uniformly accelerated any how, in repeated vibrations, that there may be no sensible diminution of the relative velocity, the whole will be exerted on the weight to be raised; *i. e.* the tension of the rope or chain, by which the power is confined to act on the weight, will always be the same as tho' the Beam were at rest; and then, by expunging ma^2 out of the expression for the greatest effect, $n \sqrt{\frac{ma - nb}{ma^2 + nb^2}}$ becomes evidently enlarged to $n \sqrt{\frac{ma - nb}{nb^2}}$. The consequences are these. 1st, The greatest effect of this engine when m is a spring, will always exceed the cotemporary effect where m is a weight. 2dly, The proportion of the Power and weight will then be

be $n : m :: a : 2b$, as appears by taking the fluxion of $n \sqrt{\frac{ma - nb}{nb^2}} = 0$, and reducing the equation in the manner above. Whence the load to be raised for the greatest effect of a steam-engine, if the Inertia of the materials composing its working parts be put out of the question, will be just half of what is sufficient to balance the atmosphere, whether the Brachia of the lever be equal or not.

Permit me now to trouble you with two or three remarks on what I formerly laid before you concerning the proportion of the cylinders. And, 1st, in all values of the Brachia, with regard to their lengths, and all values of n , the expression $\sqrt{\frac{ma^2 + nb^2}{ma - nb}}$ for the time of a stroke, when m is a weight, is the general expression to be used for the time. 2dly, m being considered as a spring, the time of a stroke is as $\sqrt{\frac{nb^2}{ma - nb}}$; and then if, according to what I have there directed, a be taken variable, and m the reciprocal of a , the advantages to be gained by the breadth of the cylinder can only arise from a diminution of friction, and from the matter in the Beam; for, the expression $\sqrt{\frac{nb^2}{ma - nb}}$ becomes constant, and thence the strokes are isochronal. I might, furthermore, proceed to examine into these advantages, more explicitly than is there done, upon the principles laid down, when m is a weight. But many particulars (such as the form of the Brachia and various appendages, with their quantities of matter and centers of gyration)

gyration) being wanting to perfect the Theory of the construction, I shall drop the inquiry when I have made only one remark more. It is this. The shortness of the Brachia diminishes the resistance of the Engine to motion; and, therefore, the inequality which I proposed in them was in part to avail myself of that obvious advantage, without incurring the inconvenience of enlarging the pump-bores. I say it is an obvious advantage; for, the matter in the Brachia, that the Equilibrium may be preserved, being inversely as their lengths, and the resistance to motion in the direct ratio of the squares of those lengths, the resistance of the longer arm is to that of the shorter as the lengths of them directly.

Queen-square, Westminster,
June 22, 1756.

II. Observations on the Growth of Trees: By Robert Marham, of Stratton in Norfolk, Esq; communicated by the Rev. Steph. Hales, D. D. F. R. S.

Measures of Trees, taken in April 1743, before they began to shoot; and again in Autumn 1758, after the Year's Growth was completed. The Measure taken at 5 Feet from the Earth.

FIRST TABLE.

Read Jan. 11,
1759.

1. Ash, planted since 1647 —
2. Oak, past thriving, but found —
3. Oak, about 80 years old —
4. Scotch Fir, feed in 1698 —
5. Oak, planted above 60 years —
6. Spanish Chestnut, near 60 years old —
7. Another, 45 or 46 years old —
8. Oak, planted by me in 1720 —
9. Scotch Fir, planted 1734, 2 feet high —
10. Pinaster, planted in 1734 or 1735 —
11. Oak, set an acorn in spring 1719 —
12. Oak, planted in 1720 or 1721 —

	Circumf. in Spring 1743.		Circumf. in Autumn 1758.		Increase in 16 years.		Content in 1743.		Content in 1758.		Solid increase in 16 years.	
	Feet.	Inches.	Feet.	Inches.	Feet.	Inches.	Cubic feet.	Quarters.	Cubic feet.	Quarters.	Cubic feet.	Quarters.
1.	9	10	4	11	1	0	09	1	318	76	3	43
2.	6	4	4	4	1	0	54	1	336	63	2	80
3.	6	3	3	6	1	0	24	1	284	36	2	408
4.	5	4	4	6	1	1	17	3	48	26	1	270
5.	5	11	1	7	2	3	21	3	224	32	0	174
6.	4	4	0	5	6	3	11	2	408	18	3	270
7.	2	9	2	4	1	6	4	2	391	11	2	408
8.	2	11	2	5	2	2	5	1	126	16	0	260
9.	1	11	6	4	0	2	3	2	260	10	0	0
10.	2	5	1	4	3	1	1	2	116	4	1	68
11.	1	7	0	2	1	1	1	2	116	4	1	36
12.	2	9	5	4	1	11	4	2	391	14	0	174
							213	0	300	322	0	333
											109	0
												33

Now as the twelve trees above, contained 213 cubic feet 300 inches of timber in spring 1743, and have increased to 322 cubic feet 333 inches in autumn 1758; that is, 109 cubic feet 33 inches in 16 years growth; if all the trees were of the same kind, 109 feet pays 3 *per cent.* for standing: and the six oaks pay near the same interest, although one of them, N^o 2. appeared past thriving in 1743; for the increase of the six oaks is from 112 feet 1 quarter 171 inches of timber, to 167 feet 138 inches, *i. e.* 54 feet 2 quarters 399 inches; which is above 3 *per cent.* But if you take only the five thriving oaks, then their content is, from 57 feet 3 quarters 267 inches, to 103 feet 2 quarters 58 inches; *i. e.* 45 feet 2 quarters 223 inches of timber; or near 5 *per cent.* And the increase of the most thriving oak, N^o 8. appears, by the above table, to pay above 12 $\frac{1}{2}$ *per cent.* and the Scotch fir, N^o 9. being under 2 feet and half of timber in spring 1743, and 10 feet in autumn 1758, pays above 18 $\frac{1}{2}$ *per cent.* Besides it should be considered, altho' I measured the largest and most thriving oak and Scotch fir in 1743, yet several others of the same age, both oaks and Scotch firs, have greatly exceeded the measured trees for many years past; *e. g.* the oak N^o 11. appears by the table two feet 8 inches 2-8ths in circumference; and another just by it is 2 feet 11 inches 6-8ths; and an oak transplanted from this grove, is 3 feet 9 inches 5-8ths round; yet this last tree was considerably less than the first when removed, and not planted in a better soil, and yet is 1 foot 1 inch 3-8ths larger than the original tree. The first contains 4 feet 1 quarter 336 inches, and has gained 2 feet 3 quarters 220 inches

inches in sixteen years: the last contains 8 feet 3 quarters 68 inches; and, supposing them equal in 1743, gains 7 feet, 384 inches; *i. e.* above $2\frac{1}{2}$ the increase of the first tree. But notwithstanding the transplanted oak is thus much larger than the original oaks in the grove, yet as the transplanted tree does not run half the height of the trees in the grove before it heads, they differ but little in their quantity of timber.

The following table shews the monthly increase of trees in the years 1757 and 1758. As I endeavoured to take the measures with as much exactness as was in my power, I cut three, four, or more notches in the bark of each tree, that my line might always be confined exactly to the same place. I observed, if I measured soon after a rain, whilst the bark was saturated with water, the tree would be $\frac{1}{8}$ of an inch larger than after a day or two of dry weather. I may here add, that all the measures of circumferences of trees are taken at 5 feet from the earth: and consequently the solid measures must include 10 feet in length. I generally made use of Keay's Tables in the solid measures, which go no lower than quarters of inches in girts: which is not so exact as it ought to be.

Measures of the monthly Increase of the most thriving Trees I had, of the following several Kinds,
taken at 5 Feet from the Ground in the Years 1757 and 1758.

TABLE II.

	Feet, Inches, Grains of In	1 June 1757.	3 July 1757.	3 Aug. 1757.	4 Sept. 1757.	Nov. 1757.	1 June 1758.	3c June 1758.	31 July 1758.	29 Aug. 1758.	29 Sept 1758
1. An Oak planted in 1720, No. 8. }	0 9 7	4 10	4 10 3	4 10 7	4 11 2	4 11 2	4 11 3	4 11 6	4 11 6	5 1 2	5 1 7
2. A Beech, a seed in 1741. }	0 11 0	0 11 1	0 11 5	0 11 1	0 11 4	0 11 4	0 11 4	0 11 4	0 11 4	1 2 2	1 2 3
3. A Scotch Fir, planted in 1734. }	4 2 4	4 3 0	4 3 1	4 3 2	4 3 4	4 3 6	4 3 6	4 3 6	4 3 6	4 4 4	4 4 6
4. An Oak, an acorn in spring 1719. }	2 9 4	2 9 5	2 9 6	2 10 1	2 10 2	2 10 2	2 10 2	2 10 2	2 10 2	2 11 1	2 11 6
5. A Spanish Chestnut 45 or 46 years }	4 2 5	4 2 7	4 3 0	4 3 1	4 3 4	4 3 4	4 3 4	4 3 5	4 3 5	4 4 1	4 4 4
6. An Elm, about 25 years old. }	1 7 0	1 7 4	1 8 0	1 8 2	1 8 4	1 8 4	1 9 0	1 9 4	1 9 7	1 10 2	1 10 3
7. A Spruce Fir, planted in 1735. }	1 0 7	1 1 5	1 2 2	1 2 5	1 2 7	1 3 1	1 3 6	1 4 3	1 4 6	1 5 1	1 5 1
8. A Larch, planted in 1749. }	1 0 7	1 1 5	1 2 2	1 2 5	1 2 7	1 3 1	1 3 6	1 4 3	1 4 6	1 5 1	1 5 1
9. A Willow, set in spring 1756. }	1 0 7	1 1 5	1 2 2	1 2 5	1 2 7	1 3 1	1 3 6	1 4 3	1 4 6	1 5 1	1 5 1
10. A Beech, a seed in 1733. }	1 0 7	1 1 5	1 2 2	1 2 5	1 2 7	1 3 1	1 3 6	1 4 3	1 4 6	1 5 1	1 5 1
Measured October 6, 1756; and on the 6th											
of November they were rather less.											
Most of these trees were 1-8th of an inch less											
in the beginning of April 1757, than at their											
last measure; and had not increased on the											
1st of May; but on the 1st of June flooded thus-											
July 3, 1757, the last month was very dry.											
August 3, 1757, the first half of the last											
month was absolutely dry and hot, and the											
last half frequent showers.											
September 4, 1757, from 3d Aug. to 10th											
very hot; the rest of the last month much											
cooler; and the last half frequent showers.											
November.											
June 1st, 1758, very dry spring.											
June 30, 1758, last month rather dry.											
July 31, 1758, last month very rainy, but hot.											
August 29, 1758, last month hot, and rather											
moist.											
Sept. 29, last month very wet and cool.											

TABLE III. Shows the Increase in Circumference, and in solid Measure, of each Tree in 1758.

	Circumfer. in 1757.			Circumfer. in 1758.			Content in 1757.			Content in 1758.			The year's solid Increase of 1758.			Interest the Trees pay for standing.	
	Feet.	Inches.	8ths of In.	Feet.	Inches.	8ths.	Cubicfeet.	Quarters.	Inches.	Cubicfeet.	Quarters.	Inches.	Cubicfeet.	Quarters.	Inches.	'	Per Cent.
1. The Oak	4	11	2	5	1	2	15	0	188	16	0	260	1	0	72	or near	7
2. The Beech	1	0	4	1	2	3	0	2	216	0	3	174	0	0	390	above	36
3. The Scotch Fir	4	3	6	4	4	9	11	1	68	11	2	408	0	1	340	about	4
A Scotch Fir, not in the 2d Table, planted in 1735																	
4. The Oak	2	10	2	2	11	6	5	0	30	5	2	216	0	2	186	above	12½
5. The Spanish Chestnut	4	3	4	4	4	4	11	1	68	11	2	408	0	1	340	near	4
6. The Elm	1	8	4	1	10	3	1	2	408	2	0	174	0	1	198	near	21
7. The Spruce Fir	3	6	2	3	8	1	7	0	270	8	1	264	0	2	426	above	9½
8. The Larch	1	3	1	1	5	1	0	3	392	1	1	8	0	1	48	above	28½
9. The Willow	5	2	3	5	4	3	16	2	318	17	3	48	1	0	162	above	6½
10. The Beech	1	10	4	2	0	5	2	0	174	2	2	0	0	1	258	above	19
Weymouth Pine	1	6	2	1	9	0	1	1	270	1	3	84	0	2	14	about	36

B. I measure the oak N° 4. as three feet round, as it wants only 1-4th of an inch of that measure; and the Weymouth pine as 1 foot and 6 inches, tho' it is 1-4th of an inch more.

As the Scotch fir, N° 3. has been fickle for two years past; therefore I add another Scotch fir (one year younger) to show the growth of that kind of tree; and the extraordinary increase of the Weymouth pine increased me to put that in also, tho' I had not measured it monthly.

The great Lord Bacon says, "*the improvement of the ground is the most natural way of obtaining riches.*" What great fortunes might be raised, by those that have property, in the vast heaths and downs, or fields of poor land, in this kingdom, by planting parts of them? which would also add great beauty to the country, and render the dwelling much more comfortable to the neighbourhood, by the shade in summer, and warmth in winter. Some parts of these great wastes would produce good oak; and where the soil is moist, poplar, alder, and other aquatics, would be very profitable to the planter. The chalky soil seems the least promising; yet beeches sometimes thrive well upon it. The fir kind, especially the Scotch fir, will grow surprisingly upon poor sandy land; but woods of fir should be guarded with an out-line of birch and beech, to break the force of strong winds. Birch, being the quickest grower, will best protect the young fir; but as birch, after a few years, is easily blown down, so beech will be wanted to defend the firs as they become large: for I have seen broad glades made by the wind through great woods of fir in Switzerland: which, perhaps, might have been prevented, at least in part, by an out-line of beech.

I know some think, that poor land cannot produce large trees; yet the oak at Northall in Hertfordshire, whose beautiful head spreads a circle of above 40 yards diameter, stands on a dry and deep sand; and the fine chestnuts and beeches by Mr. Naylor's grand castle of Herst Monceux in Suffex, grow in a light sandy soil: and I have found, by experience, the Weymouth, Scotch, spruce, and silver firs, which I planted in a poor sandy soil, are larger and finer trees, than others set at the same time in much better land. Perhaps it may require a rich clay to produce such trees as the noble grove of oak in the Earl of Powis's park by Ludlow, or Lord Ducie's vast chestnut at Tortworth, in Gloucestershire, which I measured $46\frac{1}{2}$ feet in circumference at near 6 feet from the ground.

Although these slight observations are not so deserving the attention of the Royal Society as I could wish; yet they may possibly be the means of producing better; and for my own part, I shall always esteem it a great honour that they were communicated by Dr. Hales.

R. Marshall.

Fig 1.

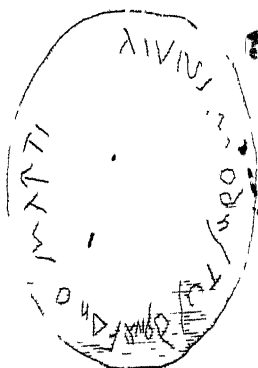


Fig 2.



Fig 3.



Fig 4.



Fig 5.



Fig. 7.



Fig. 8.

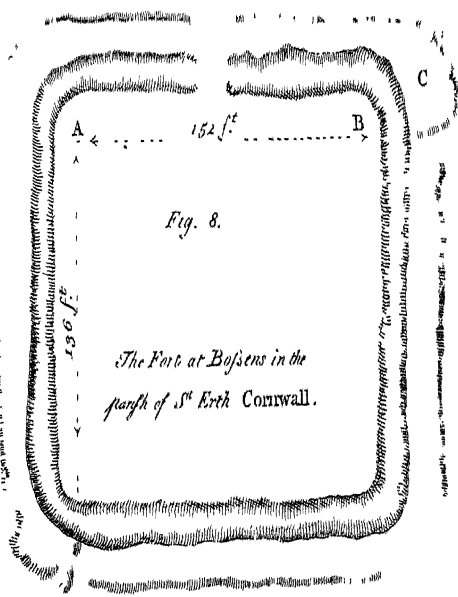


Fig. 6.



A Scale of Inches from Figure 2 to 7.

J. H. M. f.

III. *An Account of some Antiquities found in Cornwall: In a Letter from the Rev. William Borlase, M. A. F. R. S. to the Rev. Charles Lyttelton, L. L. D. Dean of Exeter.*

Rev. Sir,

Ludgvan, Dec. 4. 1758.

Read Jan. 11,
1759.

AS I know the pleasure, which every branch of ancient literature gives you, I should not excuse myself, if I did not communicate to you a late discovery of Roman antiquities in these westernmost parts of Britain.

In the year 1756 a farmer at Boffens, in the parish of St. Erth, driving his oxen from the field, perceived the foot of one of them to sink a little deeper than ordinary into the earth at A, *fig. 8.* (*See TAB. I.*) Curiosity, and the hopes of treasure, led him soon after to search the place; where was soon discovered a perpendicular pit, circular, of two feet and half diameter. Digging to the depth of 18 feet, there was found a Roman patera (*fig. 1. & 2.*): about 6 feet deeper, the jug, *fig. 3*: near by, among the rubbish, the stone, *fig. 4*; a small millstone, about 18 inches diameter: then another patera, with two handles, in other particulars of the shape and size as *fig. 2.* but unfortunately mislaid, and not now to be found. Intermixed with these were found fragments of horns, bones of several sizes, half-burnt sticks, and many pieces of leather, seemingly shreds of worn-out shoes. Having sunk to the depth of 36 feet, they found the
bottom

bottom of the pit concave, like that of a dish or bowl. There was a sensible moisture, and mostly wet clay, in all parts of the pit. On each side there were holes at due distances, capable of admitting a human foot, by which persons might descend and ascend. There is no doubt but this work must have been intended for a well: but a pit so deep, and of such narrow dimensions, must have been sunk thro' a stony ground with much difficulty, and with tools very different from those now in use.

Coming to the spot on the 22d of May last with Henry Davies, Esq; proprietor of the land, who first favoured me with the notice of this discovery, I found, on the higher part of the tenement, in a field called the Rounds, the remains of a fort: the length of it, bearing nearly north and south, was 152 feet; the breadth, from east to west, about 136 feet. The foss on the outside is still discoverable; the walls dismantled, but sufficient remains to shew, that the work was rectilinear, with the angles rounded off; a manner of fortifying, which the Romans were generally fond of, as may be seen by their stations *per lineam valli* (Horsley *Britannia Romana*, p. 113, and many other places). At the north corner, B, there was an additional building, projecting outwards beyond the rampart, about 30 feet long, not quite so wide: at the south angle at D there are the signs of a building of like kind: these were the procestria of the fort. The shape and size of the work, as it stands at present, may be seen in the drawings annexed, *fig. 8.*

Upon examining the rubbish near the pit, I found the cut stone, *fig. 5.* part of a large stone vase, and part

part of an earthen sepulchral urn : I found also some fragments of leather : all which, with what was found before, and brought me, I shall beg leave to describe, with a few observations.

Fig. 1. and *2.* are two views of the patera : it was made of tin, the 20th of an inch thick, four inches and a half wide at the brim, but growing narrower downwards, was at the bottom, which was flat, two inches and a half in diameter. The bottom of the inside is represented, *fig. 1.* in its real size. *Fig. 2.* is the side of the same patera, by the scale annexed. The Roman patera was not always of the same dimensions. When it was of the larger size, its use is well known to have been for receiving the blood of the victim, or to be carried before the priests with other offerings ; but when of smaller dimensions (as this is), either to offer libations of water, oil, or wine, on the altar (whence on medals the hand so often stretched out, holding the patera towards the altar), or to participate the rites of sacrifice by drinking. This patera had no ansa, or handle ; tho' that which is lost, and has been mentioned above, had one on each side : and indeed those found in England generally have. Mr. Addison observes [*Travels*, of his works, p. 115.], that it is not so common to find pateræ with handles to them abroad ; but that a patera without a handle would be as singular here [in England] as one with it at Rome : and Mr. Horsley, (p. 191), that all pateræ, which he had seen upon any altars in Britain, had handles, tho' of different sizes and shapes : but it may be observed, that the five pateræ, which I have seen found in Cornwall, never had any, and are therefore the more remarkable. It

is more rare still to find these seemingly trifling cups and dishes inscribed to a particular deity; but most uncommon to see them distinguished by the names of the donor and his father, as well as the name of the deity to which they were dedicated. This patera, found at Boffens, about three miles north-east of St. Michael's Mount, is a singular instance of the latter usage, and has the following inscription engraved on its bottom, in shape, and size, and circular line, as in *fig. 1.*

ΛΙΒΙΟΥ ΜΟΔΕΣΤΟΥ ΔΟΥΛΙ Γ ΔΙΟ ΜΑΡΤΙ

Which I read thus, till better information. *Livius Modestus Driuli* (or *Douiuli*) *f.* (for *filius*) *Deo Marti.*

The two first words are very plain (tho', like the whole, a mixture of Greek and Roman characters), and not rare in Roman history. *Livius* is too well known to need any comment; and as it is well known, that the virtues oftentimes gave names to persons, it is as certain, that modesty among the rest had that privilege. Sometimes the person, who had this name, was called *Verecundus* [*Diis Manibus Verecundi*, in a Roman monument at Skirway in Scotland; *Horfley*, 199: and the same name is to be traced in another monument, *Ibid.* Plate 64. N^o. X.] Here it is *Modestus*, of which we have also instances, as *Publius Ælius Modestus Præfectus*, who dedicated an altar to Hercules, *Ibid.* Pl. 16. N^o. XLI.; and *Caius Murrius Modestus miles* in Somersetshire, *Ibid.* Pl. 71. N^o. II.

The

The letters in general of this inscription are badly shaped; but in the third word particularly the characters are more perplexed than in the rest. The first letter is the Greecian small *delta*; the second I take to be the little *ro* of the Greeks reversed (*viz.* with the long stroke on the right, instead of its being as usual on the left hand): the other letters are more truly delineated, tho' somewhat crowded: so that I take this word to be *Driuli*, a name, which I do not recollect to have met with before: but if the second letter shall appear to the learned to be more likely intended for the Greek character *u*, or *ou*, this word will then be *Douiuli*, or *Duili*, a name very honourable among the Romans. *F* stands for *filius*, as usual; and the two last words are beyond doubt *Deo Marti*. The language is Roman; but the *λ*, *δ*, *Δ*, *η* (which in the words *Modestus* and *Deo* is used for the Latin *e*), and the *ro*, or *q*, are proper to the Grecian alphabet. The *R* in *Marti* is singular, intended for *P*, the Greek capital *Ro*; but instead of the semicircular part joined to the upright, thro' the incorrectness of the engraver, it has a demi-hexagon, like a canopy, over the upright line. The *o* is oval, not round as with the Latins; and the *A* has no transverse stroke. The other letters are common to the Greeks and Romans.

That this Latin inscription should have so much of the Greek character is remarkable. It is well known, that the Druids used the Greek letters. Whether the person, who consecrated this patera to Mars, might intrust the engraving to one of the Druid sect, or whether the engraver was one of the auxiliary cohorts and natives of Greece, as the Thracian and

Dalmatian horse (for such we certainly had in Britain), it is in vain to inquire; but neither of these conjectures is improbable. There are, I think, but two inscriptions in the Greek language as yet found in Britain; but in the Latin language this seems to me the only one as yet discovered in the island written in Greek characters.

Fig. 3. is a jug or jar (of tin also) containing four quarts one pint and $\frac{3}{4}$ of a quatern, wine measure: its weight 7 lb 9 $\frac{1}{2}$ oz. It is the *præfericulum* of antiquarians, a vessel used to bring the holy water, or other sacred liquor, to the altar. It seems to have had that name from its being carried in procession before the priests in a kind of shallow basin (which Festus chuses rather to call the *præfericulum*, as see Montfaucon *de pateris*, tom. ii.), in much the same manner as the basin and ewer were formerly used among us.

Fig. 4. & 5. are of stone. The first and largest weighs 14 lb 1 oz. (avoirdupois) and 11 *dwt.* amounting, if I mistake not, to 18 lb Roman and 337 grains. The second and smaller stone weighs 4 lb 1 oz. (avoirdupois) and 7 *dwt.* or 5 lb $\frac{1}{4}$ Roman and 95 grains. By the holes these stones have near the top, they were probably designed as weights, whereby provisions were bought for, and afterwards shared among, the soldiers of the fort.

The ancients sometimes made ~~their~~ weights of stone, and of different shapes, round, rectangular, and conical: sometimes they were of marble, as those exhibited by Gruter, p. 221, 222. (as Kemp quotes him), and that in the *Monumenta Kempiana*, p. 152. and in other repositories. These here are
both

both of the dove-coloured Cornish granite, discoloured as it seems by fire. But the ancients seem not to have made their weights of stone by choice, but for want of metal; for they could not be ignorant, that lead, brass, or iron, was more compact, ponderous, and durable; and that stone was liable to become heavier, by assuming into its substance the moisture and weight of adjacent bodies, and on the other hand to be corroded, and become more porous and lighter, by means of any penetrating acid, or heat or drought; and so either exceed or fall short of the intended standard. These inconveniences of stone weights the ancients, I say, could not but foresee; they are alterations which the materials could hardly escape: and therefore I conjecture, that the weights before us have varied: they both exceed the number of Roman pounds, which they were probably at first adjusted to. The great weight indeed has been increased but a small matter, *viz.* 337 grains more than 18 pounds; the small one more in proportion, *viz.* 1406 grains more than 5 pounds, *i. e.* above a quarter of a pound: nor is it to be wondered at, that, tho' both of the same sort of stone, they should have acquired different quantities of weight; for the addition must have been according to the nature of the rubbish in which they have lain. If there were any metallic ores (iron, for instance, copper, or tin) or impregnated waters, in the rubbish, where these stones were deposited, then the addition would be great, otherwise the addition would be no more than that of common moisture or earth.

Fig. 6. is part of a vase or bowl, sometimes made of brass, or richer metal, but here of stone. This,

I apprehend, is what Festus calls the præfericulum [*Præfericulum, inquit Festus, vas æneum, sine ansâ, appellatum patens summum, ut pelvis, quo ad sacrificia utebantur in Sacratio.* Montfaucon, tom. ii p. 142.); and by its gradually increasing thickness towards the bottom, appears to have been of like design with that exhibited in the *Antiquities of Cornwall*, plate xxi. fig. 5. p. 274. This vase was of curious grey granite, formed by turning, well polished within, somewhat discoloured without, as if it had suffered by fire.

The small millstone, by the smoothness of one side, shews that it had been much used; and was such, without any material difference, as is now used in the islands of Scilly (and elsewhere) for hand-mills to grind corn in times of siege and confinement, and must be absolutely necessary in all forts.

The bones and horns may be supposed to have belonged to animals, either sacrificed, or killed for the sustenance of the garrison: the ashes, and half-burnt sticks, are the remains of sacred or culinary fires. The fragments of leather are for the most part patched, and coarsely sewn together; but one piece, which I found more entire, may contribute perhaps to shew us the shape of the Roman calceus of those times; and may be seen *fig. 7.* by the same scale with the rest. Some bits of leather were also pierced with circular holes; but whether parts of the calceus, cothurnus, or any border for the habit, armour, or vehicle of the officers, enough does not remain to decide.

I shall make no other reflection at present on these antiquities, than that the inscription is the first discovered

vered in this county of such high antiquity; and will satisfy the learned, that the Romans had penetrated into the westernmost parts of Cornwall before the empire became Christian: that the sacrificial vessels, the pateræ, and præfericulum, are of tin, the natural product of Cornwall: the vase, the weights, the millstone, are also of Cornish granite: and by the walls, the religious utensils, the weights, the quantity of shoes, bones, horns, vases, urn, and ashes, this fort appears to have been that of a fixed garrison, not a temporary occasional fortification: that by the shape of this fort, and the antiquities discovered in it, it was a Roman fort.

I remain,

S I R,

Your most obedient Servant,

William Borlase.

IV. *A new improved Silk-Reel. By the Rev. Samuel Pulein, M. A.*

Read Feb. 1, 1759. **T**HE following paper, it is hoped, will help to promote the culture of silk in our American colonies, and to bring it to that perfection, which at present is scarce found in any country but Piedmont.

When silk is reeled from the cocoons, the thread is smeared with the natural gum of the silk softened by the heat of the water out of which it is reeled.
If

If the several rounds of this thread, as it falls on the reel, touch one another in their whole extent, it is then so glued together, as to be intirely usefess for any perfection of manufacture, it being impossible to wind it off without tearing and breaking. This fault in the filk countries goes under the technical name of the Vitrage ; and there are many degrees of it, according to the cause by which it is produced.

From the introduction of filk into Europe to this day, the preventing of the Vitrage has employed some of the most knowing men in the countries where filk is produced.

To change the position of the filk thread, that it might not always fall on the same part of the reel, the guidestick was introduced. This received a progressive and regressive motion, by means of two wheels, the one fixed to the axle of the reel, and communicating its motion to the other by means of a band. To make these wheels perfect, certain determined and precise proportions are to be observed in the diameters of their grooves, otherways, notwithstanding the motion of the guidestick, there may be a total Vitrage of the whole hank of filk ; and, even when the proportion of the wheels is perfect, there may be reckoned up thirteen or fourteen accidents, any of which happening will cause a Vitrage : but tho' these inconveniencies attend all those reels which use a band, yet, because the ordinary sort of them are easily made, they are still used by the common people in most filk countries.

No nation has brought its filk to a greater perfection than the Piedmontese : but it was only by slow degrees, and in a long tract of time, that they arrived

rived at this. They first suppressed the old method of reeling the silk over a bobin, which was found to give it a flat form; and they substituted in its stead that excellent method of the *croissure*, which renders it round and compact. They then applied themselves to correct the imperfections of the guidestick, and to establish proper proportions between the wheels which gave it motion: but, notwithstanding these and other improvements, they still found, that, so long as these wheels were turned by means of a band, they could not arrive at the perfection necessary to make silk fit for organcine or warp, because the inaccurate motion of the band made the most just proportion which they could establish between the wheels quite ineffectual, and constantly produced a *Vitrage*.

- It was on this account that they totally suppressed the usage of the band, and substituted in its stead four wheels with a determined number of teeth, whose revolutions, being uniform, gave the guidestick a proper motion for preventing the *Vitrage*. This, and some other regulations, are established by laws, which are rigorously put in execution, lest the common people should thro' indolence relapse into their old customs. By adhering to this reel, the Piedmontese are able to give that perfection to their raw silk, which fits it to be thrown into organcine or warp; and to raise the value of each pound of silk reeled in this manner to one third more than it would have if reeled otherways.

The French were desirous of making raw silk fit for organcine or warp among themselves, which hitherto they have had from Piedmont. Whether they thought

thought the Piedmontese reel could not easily be brought into common use, or whether a little vanity hindered them from copying or improving the inventions of other nations, I will not determine. However that be, they applied themselves wholly to the improvement of the band-reel.

Two persons, *viz.* M. St. Priest of Languedoc, and M. Vaucançon of the Academy of Sciences at Paris, have, within these three or four last years, brought the band-reel to the greatest perfection of which it is capable. Either of their reels will make raw silk fit for organcine or warp, provided they are accurately made; but a small error in their construction destroys their perfection. I have now in my possession a reel made according to M. Vaucançon's method; where I can shew, that in two wheels, whose diameters differ but very inconsiderably, one shall reel the silk properly, and the other throw it into a total Vitrage. I have however given a description and plate of this reel, in a treatise on the culture of silk, which I published last year, because its contrivance is more simple than that of M. St. Priest, and performs as well.

The Piedmontese reel is free from the inconveniencies of all those reels which use a band: the maker cannot easily err in the construction; the weather cannot affect its operation; nor is it subject, during its work, to the many irregularities of the band-reel. The chief objections which have hindered its being commonly used are, that four toothed wheels are more difficult to be made than two plain grooved ones; and that being made only of wood, they are easily broken. So that it is necessary to

have a double set of them, in order to prevent those delays which might happen by their breaking; because any delay in the reeling of silk occasions a considerable detriment.

Our happier constitution doth not admit such rigorous laws as that of Piedmont. To make the common people adopt any new contrivance, they must not only know that it is the best, but they must also feel that it can be easily practised. Those who superintend public filatures in our colonies may indeed, for the present, keep their workmen to the Piedmontese reel; but public filatures can no more produce large quantities of silk than public spinning-houses could produce the vast quantities of linen-yarn which are now raised from private family-wheels: And when the management of silk-worms becomes more common in our colonies, and people find that, by reeling their own cocoons, their profit will be nearly doubled, and this with much less labour than was used in rearing the worms; when this, I say, shall happen (and it is to be hoped it soon will), then the making of the Piedmontese reel more simple, and more familiar to the common people, will I believe appear a thing of considerable importance to our silk manufactures.

I some time ago turned my thoughts to the effecting of this; and should before now have put them in execution, had I not distrusted my own abilities to perform a thing, which the Piedmontese themselves had not attempted. I have now constructed a reel, which, without any additional mechanism, performs the work of the Piedmontese reel, and uses but two wheels with teeth instead of four wheels. It has

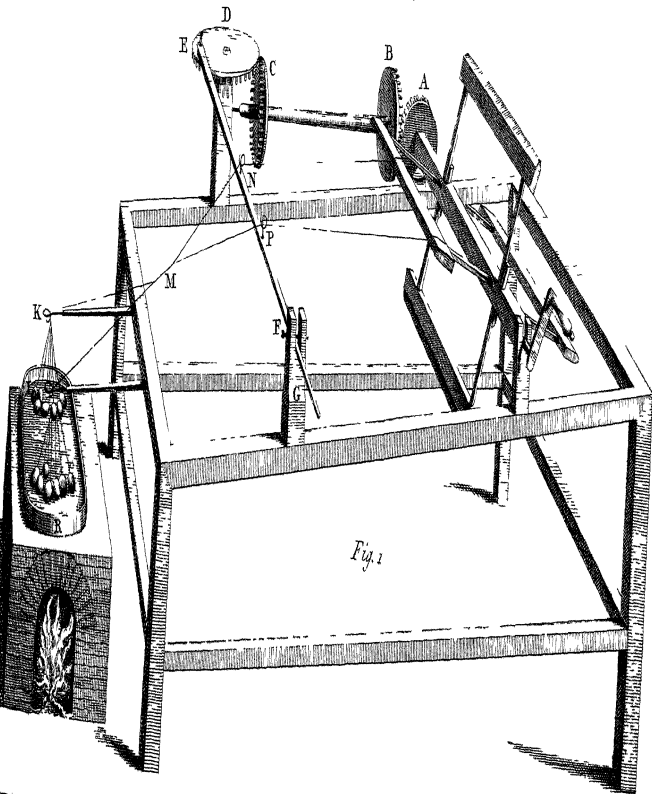
been examined by some gentlemen in the silk-trade, who are well acquainted with the rears of that country, and allowed to work with much more simplicity, and greater perfection. Nor can I account why this improvement was not before attempted, but from the strong bias of habit confining the powers of invention.

Some advantages, which this reel seems to have above the present Piedmontese reel, are, 1st, that by a new proportion struck between the wheels, the silk thread will not be laid in the same place on the reel till after the winding of near six hundred yards: by which means, the thread having time to dry, all Vitrage is avoided; whereas those now used do not reel above an hundred and fifty yards till the thread is laid in the same place.

2^{dly}, I have added to this reel an improved method of making the double croissure in as easy a manner as the Piedmontese now make the single one; the double croissure giving a considerable perfection to the silk above what it obtains from the single one.

As the limits to which I purpose confining this memoir will not allow of a more extended description, which, to gentlemen not versed in these matters, may already be thought too long, I shall avoid a greater trespass on your attention, by a brief reference to the annexed figure [See TAB. II.], and to the model before you [See TAB. III.].

Tab. II. represents the present Piedmontese reel. The position of the four wheels is shewn at A, B, C, and D: the toothed wheel A is fixed upon the axle



of the reel; and, as the reel turns, it gives motion to the two wheels B and C, which are fixed upon one common arbre. The wheel C moves the wheel D, which is placed horizontal; and this, as it revolves, makes the guidestick E F play forward and backward in a groove made in the upright G, the guidestick playing freely on a pin E fixed in this wheel. Two threads of silk, drawn from two parcels of cocoons, which lie in the copper of hot water R, are passed thro' the two loops of the stop-wires K and L: they then are twisted round each other at M, which is the single croissure of the Piedmontese. At M they are again separated, and pass each through its own guide-wire at N and P; and from these they go to the reel, where, as the reel turns, the motion of the guidestick continually varies their position, and hinders them to be laid on the same place.

The model before you, represented by *Tab. III.* exhibits the new construction which I have given to this reel. The two wheels C and D of *Tab. II.* are laid aside as superfluous: the arbre Q passes thro' a nich in an upright supporter R, and, by a winch at its end A, gives motion to the guidestick. The plane of its motion in the Piedmontese reel is horizontal, but here it is perpendicular. This perhaps made them think that the effect would not be the same; but it doth not cause any essential difference; and if it had, there was a most easy remedy for it.

Here therefore the expence and trouble of making and adjusting two wheels and two sets of teeth are saved, the hazard of breaking and going out of order lessened, and the machine made more simple, and more familiar to the understanding.

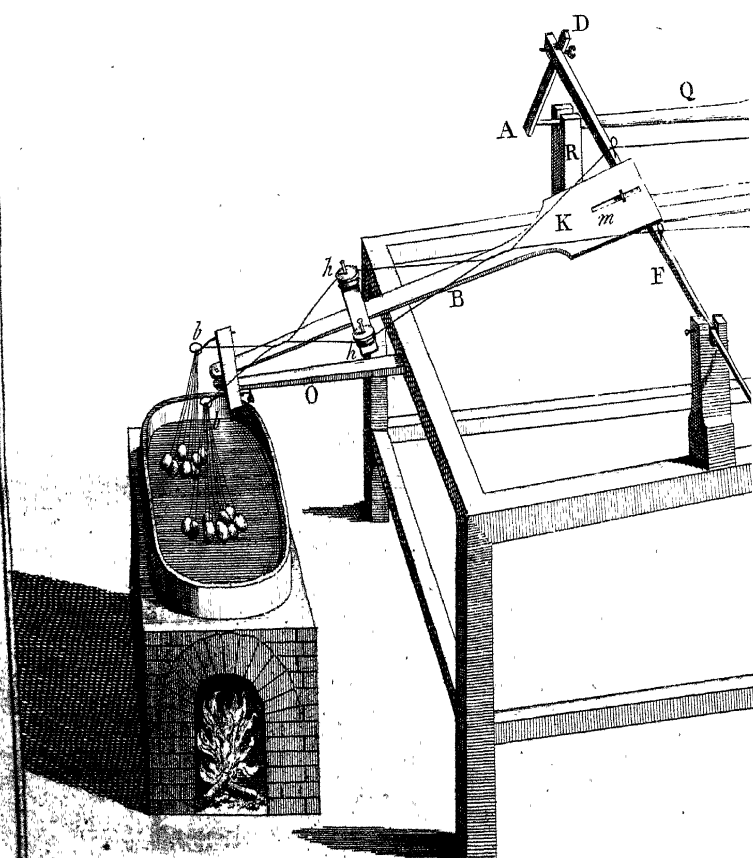
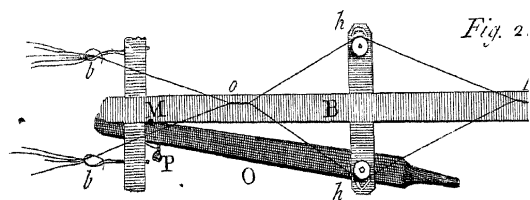
As I intend, at a convenient time, to give a more ample account of the method in which silk is reeled, and of the various attempts which have been made to improve the silk-reel, together with their success, that persons may not waste their time upon fruitless experiments; and having hopes also that I shall be able to add some further improvements to it, of which I have not as yet had leisure to make trial; for these reasons I shall for the present take leave of this subject, at least with this pleasing thought, that the pains I have taken may possibly hereafter promote the industry and profit of such poor families as are, in all countries, the supporters of the silk manufacture.

Samuel Pulein.

In *Tab. III. fig. 1.* at A is shewn the manner in which I have made a winch D, fixed in the arbre Q A, answer the same purpose as the two wheels with teeth, which are used in the Piedmontese reel; by which the mechanism is rendered more simple, and the guidestick F moved to the same advantage as with the wheels.

At *fig. 2.* is shewn the double croissure, made by means of the cross B, which I shall call a swivel-cross. It is made of very slight scantling, such as a common lath may afford. At *b b* are fixed two little ivory or brass wheels with smooth acute-angled grooves in them to receive the threads which come from the two stop-wires *b b*. One end of the swivel-cross K is somewhat broad, and rests upon the guidestick F directly in the middle between the two guide-wires *c c*. Here it moves and plays freely

on



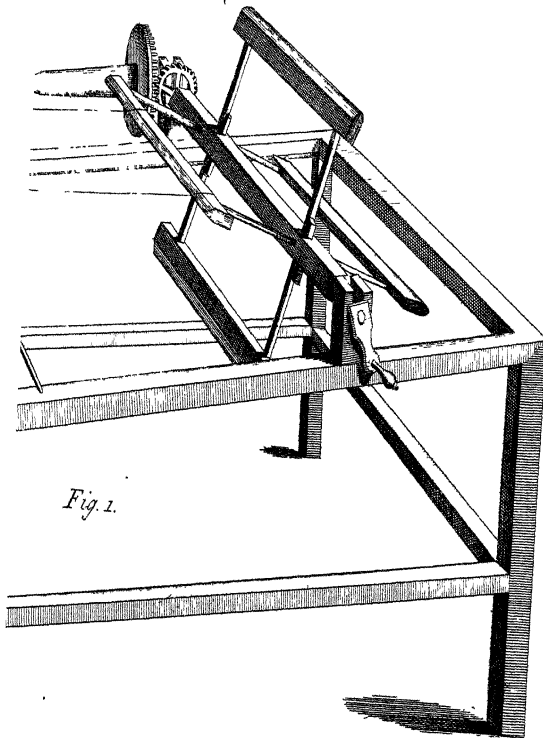
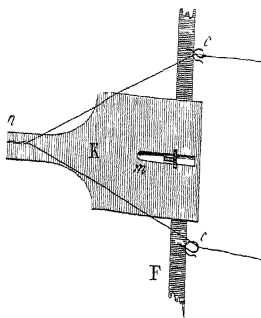


Fig. 1.

on a pin fixed in the guidestick, the pin passing thro' the nich *m*. The other end of the swivel *M* contains the stop-wires *b b*, and is attached to the end of the rod *O*, which projects from the front bench of the reel. This attachment is made by means of a piece of packthread with a knot on its end; which packthread passes thro' a hole in the swivel and in the piece *O*, and being fixed to a peg *P*, which turns in the under part of the piece *O*, is drawn to a proper degree of tension to allow the swivel to obey all the motions of the guidestick.

The manner of making the double croissure is thus: The two silk threads are passed thro' the two stop-wires *b b*, and thro' the two guide-wires *c c*, and so are fastened to the reel. Then either of the two threads is taken in that part of it which lies between the stop-wire and and guide-wire, and turned two or three times round the other thread; and then each thread is placed in the groove of the pulley at *b b*, taking care to place each in the groove of that pulley which lies on the side of its own stop-wire. The threads will then appear in the situation represented in the figure twisted round one another in the two points *o* and *p*.

The great advantage of this method is the taking off the pressure of the threads, by making them pass over two pulleys instead of two hooks; and by the swivel complying with all the motions of the guidestick, which keeps the angles of the croissure constantly the same: without this, the bearing of the threads and the variation of the angles will make them so liable to break, that, from many experiments which I have made, I might venture to say,
that

that tho' the double croiffure has many advantages in compacting the filk thread, and shaking off its moisture, yet without such contrivance it will scarce ever be put in practice. I must also take notice, that this contrivance is vastly more simple than that which Monf. Vaucançon uses, tho' his has no one of these advantages. A description of his method may be seen in the treatise which I lately published on the culture of filk, for the use of our American colonies, sold by Mr. Millar in the Strand; but I had not at that time thought of these improvements.

N.B. In *Tab. III. fig. 2.* the swivel-cross is represented somewhat large, and separate from the reel, the better to distinguish its parts; but at B, *fig. 1.* it is shewn in its true proportion and situation when the reel is ready to work.

V. *Experiments on several Pieces of Marble stained by Mr. Robert Chambers. In a Letter to the Rev. Thomas Birch, D. D. Secret. R. S. from Mr. Emanuel Mendez da Costa, F. R. S.*

Rev. Sir,

Read Feb. 8, 1759. **I** Take the liberty to address to you some notices on the art of staining or painting of marble, and the experiments I made on those pieces of painted marble produced before this Society, at their meetings on the 21st December and 11th January last.

The

The artist, whom I also introduced at the same time to the Society, is Mr. Robert Chambers, of Minching-Hampton in Gloucestershire; and at my desire he was present at the experiments I made on his said painted marbles.

But before I relate the experiments, it may not be improper to give some little historical account of the art itself: it will at least be amusing to the Society.

Kircher, in his *Mundus Subterraneus*, lib. viii. sect. 1. c. 9. p. 45 & 46. is the first author I know, who mentions it. There was, says he, an artist at Rome, who painted several pieces of marble, in an elegant manner, for Pope Urban VIII. He would not discover his art; therefore Kircher strove by many experiments to discover it: and he made colours, viz. tinctures of metals and minerals, which coloured the marble as finely as any the artist had done, and quite penetrated the stone; insomuch that a slab cut horizontally made as many pictures as pieces or sections. Kircher gives at large the process he used for making the colours; and observes, they should always be of a mineral origin: which I incline also to believe would answer much the best.

The said author (*Ibid.*) also gives another method to colour marble, by vitriol, bitumen, &c. forming a design of what you like upon paper, and laying the said design between two pieces of polished marble; then closing all the interstices with wax, you bury them for a month or two in a damp place. On taking them up, you will find, that the design you painted on the paper has penetrated the marbles, and formed exactly the same design on them. A modern
author,

author, Wallerius, in his *Mineralogy*, vol. ii. gen. 58. p. 128. also recommends this method.

In the *Philosophical Transactions*, N^o. 7. the first method of Kircher is copied. The editor however therein says, that method has not since been tried. He adds, that one Mr. Bird had for many years (he writes in 1666) found out a way to sink colours a considerable depth into polished marble; pieces whereof were shewn to King Charles II. soon after his restoration; and, being broken in his presence, it was found, that the colours had penetrated deep into the marbles; and that many works of his coloured marbles were seen at Oxford and London. But Mr. Bird's way of doing it is not mentioned.

In the *Philosophical Transactions*, N^o. 268. is a paper, intituled, "The Way of Colouring Marble." The anonymous author gives us an account of the colours, &c. he used. It is observable they are only vegetable colours. His red, he says, he extracted again from the marble, without hurting the polish, within six and twenty hours, with oil of tartar *per deliquium*; and his brown was quite discharged by aquafortis within one quarter of an hour, and the polish of the marble quite destroyed.

I shall now proceed to give an account of the experiments I made. I could not well suggest any more, as the method of colouring the marble, the materials of the colours, &c. are kept secret by the artist, Mr. Chambers.

A piece of marble, with the several colours used, on it, like a painter's pallet, being greatly saturated with aquafortis, at different times, for twenty hours,
tho'

tho' the polish of the marble was quite effaced, yet there was not the least discharge of any of the colours, nor were they any-wise dulled, &c.

N^o. 6. A deep crimson-red colour, being left twenty hours in a strong lye of common soft green soap, suffered no change; and boiled in the same lye half an hour, also suffered no change. The marble finely powdered, and aqua fortis effused over it, the marble particles were nigh destroyed; but several red particles (no doubt the colour) remained. The marble, by common calcination, *i. e.* in a common coal fire, for half an hour, is intirely discharged of its colour. We made the experiments on four other reds, and the result was much the same as abovesaid; so that this is a standard for his reds.

N^o. 5. A deep sea-green, being left twenty hours in a strong lye of common soft green soap, suffered no change; but boiled in the same lye it quite discharged its green colour: however, it yet remained slightly tintured yellowish. By common calcination the colour was quite discharged. Some other greens were tried, and answered much the same.

N^o. 10. 15. & 16. Brownish or terrestrial yellowish colours, near to a clay colour, boiled in a strong lye of common soft green soap, they suffered no change. By common calcination the colours were discharged, but retained a greyish cast. These colours, covered for forty-eight hours with a layer of the said common soap, suffered no sensible change.

N^o. 19. A bright yellow, boiled in a strong lye of common green soft soap, suffered no change; and covered with a layer of the same soap for forty-eight hours, the colour is dulled. By common cal-

cination the colours are discharged, but retain a greyish cast. Several other different shades of yellow answered much the same by my experiments.

For blue, Mr. Chambers has not as yet stained any marble of that colour.

By the above experiments we may conclude, that these colours are good, penetrate the marble freely without injuring it, remain uninjured by menstrua, &c.; and that only calcination discharges them. Therefore it is probable, that Mr. Chambers's method of staining or colouring marbles is extremely good.

Tho' acid menstrua work greatly on marble, yet it is observable these colours are not discharged by them, but only by calcination; which, as it intirely and thoroughly destroys the compages of the stone, the substances of the colours must undoubtedly at the same time be exhaled by the force of the fire. We observe a like process in the works of nature; viz. in the dendritæ; I mean, such as are on alkaline stones: for tho' the stones are utterly corroded by the acids, yet the dendritæ, however merely superficial, remain; but if calcined, the said dendritæ are immediately exhaled, and intirely disappear.

This art will not only give pleasure to the eye by regular paintings (whereas the natural colourings of marble are very irregular), but it may be very useful to blazon arms, and for inscriptions; as sculpture alone can never express colours, and chissel'd inscriptions, &c. suffer much by age: for probably a monument of marble, rightly coloured by this method, will be preserved ages from the injuries of the weather,
tho'

tho' at the same time the stone itself will be somewhat hurt or corroded by the air.

I have the honour to be,

Reverend Sir,

Your very obliged and humble Servant,

Bearbinder-lane,
7th February 1759.

Emanuel Mendez da Costa.

VI. *Observations upon the Sea Scolopendre, or Sea Millepes. By John Andrew Peyssonel, M. D. F. R. S. Translated from the French.*

Read Feb. 15,
1759.

THIS creature, in its figure, is like the Land Scolopendre, or, as Pliny says, to the hairy caterpillar, commonly called the Milleped animal. It is of the same colour, has the same arrangement of circular rings: but whereas the Land Scolopendre is flat, this is square. I counted eighty rings, which form the body and head, when it was brought to me. This sea insect was very small, and almost imperceptible. I was surpris'd, after having kept him some time, to see a round body, of a blackish green colour, like the *glans virilis*, pass out of him, which had a considerable opening, like the canal of the urethra. This gland was surrounded by two bodies or bowels, which appeared in form of a prepuce turned back; the one was yellowish, and the other whitish; each but a

line thick, stronger and larger above, and terminating below like a ligature, filled with a matter like that contained in the intestines of fishes and insects. This may be what gave occasion to Pliny, and other naturalists, to think, that these insects, finding themselves taken, throw out their bowels in order to lessen their bulk.

It is very likely, that this gland, which it pushes out, and these kinds of bowels full of the same matter, which fills them within, which in the natural state of the insect do not at all appear, but are in the inside of the animal, without any signs of their existence, being pushed outwards, increase the size of its head to ten times its common bulk; and are the intestines, which he throws out at times. Is not the gland his stomach, and these prepuces his guts, which he protrudes and retracts at pleasure? To what purpose this play or action of these parts? I profess I do not know. It cannot be to disengage himself from the machine; and this must be very small, in order to be enabled to catch these insects. If it was of glass, and of a bell-form, the protrusion of the stomach might be seen, in order to eject that matter, whatsoever it be, in this play or action; which may perhaps be hereafter discovered.

The body of the animal is square; and the four sides are armed with such prickles, as I never saw before. Also every ring has four bundles of prickles. The following is the manner of their being disposed: At the end of each ring, above the square, on each side, we see a gland, near which a bundle of prickles arises towards the squares below. This bundle seems to fill the whole ring, and to part as it were from the
center

center of the under side ; where is a hollow or separation, which passes in a right line from the head to the tail. The bundles of prickles appear round at first, and terminate in points ; but afterwards they are seen to spread out like a fan ; so that their extremities are more than four times the bigness of their bases. They contain an infinite number of prickles, which are extremely fine, loose, and brilliant, like an aigrette of glass, but more free and loose. The range of prickles underneath spreads also like fans, serving the insect as feet ; for it is upon these he stands, and moves upon them as the Scolopendre does on his feet.

Having put these Sea Scolopendres upon my fingers, they thrust a great number of their prickles into the skin, and caused a sharp pain for some hours : it was like fire upon the part. It was in vain that I rubbed and washed the part ; and tho' the prickles were broken, yet the parts, that stuck in the flesh, produced their effect, and caused the pain I felt for some hours. Afterwards it all went off without any further ill consequence.

VII. *An Account of a Storm of Thunder and Lightning at Norwich, on the 13th of July 1758. By Mr. Samuel Cooper *. Communicated by Mr. Joseph Warner, Surgeon of Guy's Hospital, and F. R. S.*

Read Feb. 15, 1759. **A**BOUT four o'clock on Thursday

afternoon, July 13th 1758. a short but severe thunder-storm, with lightning, fell upon the top of an house standing alone, and belonging to a common garden, on the causeway near Sandling's-ferry, in the city of Norwich; struck off the tiles of the roof at the east end, to the space of a yard or two; burnt a very small hole in the middle of a lath, in piercing into the chamber, and then darted to the north-east; ript off the top of an old chair, without throwing it down; snapt the two heads of the bed-posts, rent the curtains, drove against the wall (the front of the house stands due north-east), forced out an upright of a window frame a yard long, three inches broad, and two thick; smote it in a right line into an opposite ditch, ten or twelve yards distant: then struck down on the wall of the chamber, paring off half a foot's breadth of its plaistered covering quite down to the floor; lifted up a board of the floor, and leaving an hole of half an inch diameter, pierced thro' by the side of the main

* This account is confirmed in almost every circumstance by another communicated to the Royal Society in a letter from Mr. William Arderon, F.R.S. to Mr. Henry Baker, F.R.S.

beam into the kitchen, towards the west end of a pewter-shelf; traversed the whole shelf to the east, and melted superficially to the breadth of a shilling six pewter dishes, two plates, and a pewter bason, all standing touching one another: two of the dishes were thrown down, the rest not displaced. Under this a narrower shelf of pewter plates untouched. In its descent to the floor, knocked down, as she expresses it, an ancient woman sitting in the passage westward of the shelves; who being presently taken up, her shoulders and back were found to be scorched all over, with the hind part of her left leg: the skin almost universally red and inflamed, rimpled in two or three places, but not broken: her shift burnt brown, stocking singed, with its colour of the inside discharged, and the outside unchanged: right foot very painful and bruised, with that shoe struck off, and its upper-leather torn: her gown and other cloaths without any damage. It passed through the same passage without injuring another old woman sitting knee to knee with her companion; but keeping its direction to the north-east, turned on a right angle upon the outer door, split it, and passed through into the open air. On a right line with this passage to the west, and under the same roof, is the wash-house, where stood the master and his man. They saw the woman tumble down, and heard such a violent explosion, that made them both think the whole house must come down: and the man says, with such a blaze, as if all was on fire; but that was but for a moment. To the east of the pewter shelves, and under that part of the roof, where it entered, it rushed into the kitchen closet, by tearing off a wooden but-

ton

ton, that was nailed on, and there took some pieces from a Delft dish without throwing it down, broke a quart mug, and from a four-ounce phial half full of oil cut off its empty half part without spilling a drop of the oil. The activity of the lightning was with abated violence to all other points of the compass; but not without some considerable degree of force; for it scraped the plaister off the wall in many different and distant places, both in the chamber and kitchen: and to the south-west of the chamber, where was the window, broke many panes of glass, and tore the lead outwardly, without melting it; and broke two panes of the kitchen window, with its lead, situated under the chamber window. Both kitchen and chamber smelt as strong of sulphur some hours after, as if fumigated with brimstone matches.

Sam. Cooper.

VIII. *Experiments concerning the Encaustic Painting of the Ancients. In a Letter to the Right Honourable George Earl of Macclesfield, President of the Royal Society, from Mr. Josiah Colebrooke, F. R. S.*

My Lord,

Read Mar. 1. 1759. **T**HE result of experiments (whatever the success attending them may be), in philosophical or mechanical inquiries, is not below the attention of the Royal Society.

The

The art of painting with burnt wax, (as it is called) hath long been lost to the world; the use of it to painters, in the infancy of the art of painting, was of the utmost consequence, drying oil being unknown, they had nothing to preserve their colours entire from the injury of damp, and the heat of the sun; a varnish of some sort was therefore necessary; but they being unacquainted with distilled spirits, could not, as we now do, dissolve gums to make a transparent coat for their pictures; this invention therefore of burnt wax supplied that defect to them, and with this manner of painting, the chambers and other rooms in their houses were furnished; this Pliny calls *encaustum*, and we encaustick painting.

The following experiments which I have the honour to lay before your Lordship and the Society, were occasioned by the extract of a letter from the Abbé Mazeas, translated by Dr. Parsons, and published in the second part of the 49th volume of the *Philosophical Transactions*, N^o 100, concerning the antient method of painting with burnt wax, revived by Count Caylus.

The Count's method was,

First, To rub the cloth or board designed for the picture simply over with bees-wax.

Secondly, To lay on the colours mixed with common water; but as the colours will not adhere to the wax, the whole picture was first rubbed over with * Spanish chalk, and then the colours are used.

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Thirdly,

* Spanish chalk is called by Dr. Parsons, in a note, *Spanish white*; this is a better kind of whitening than the common, and was the only

Thirdly, When the picture is dry, it is put near the fire, whereby the wax melts, and absorbs all the colours.

EXPERIMENT I.

A piece of oak board was rubbed over with beeswax, first against the grain of the wood, and then with the grain, to fill up all the pores that remained after it had been planed, and afterwards was rubbed over with as much dry Spanish white, as could be made to stick on it; this, on being painted (the colours mixed with water only), so clogged the pencil, and mixed so unequally with the ground, that it was impossible to make even an outline, but what was so much thicker in one part than another, that it would not bear so much as the name of painting; neither had it any appearance of a picture: However, to pursue the experiment, this was put at a distance from the fire, on the hearth, and the wax melted by slow degrees; but the Spanish white (tho' laid as smooth as so soft a body would admit, before the colour was laid on) yet on melting the wax into it, was not sufficient to hide the grain of the wood, nor shew the colours by a proper whiteness of the ground, the wax in rubbing on the board, was unavoidably

only white that had the name of Spanish annexed to it, that I could procure, tho' I enquired for it at most if not all the colour shops in town.

My friend Mr. Dacosta shewed me a piece of Spanish chalk in his collection, which seemed more like a *cinolia* (tobacco pipe clay), and was the reason of my using that in one of the experiments.

thicker

thicker in some parts than other, and the Spanish white the same: on this I suspected there must be some mistake in the Spanish white and made the enquiry mentioned in the note * pag. 41.

To obviate the inequality of the ground in the first experiment;

EXPERIMENT II.

A piece of old waincoat (oak board) $\frac{1}{4}$ of an inch thick, which having been part of an old drawer, was not likely to shrink on being brought near the fire; this was smoothed with a fish-skin, made quite warm before the fire, and then with a brush dipped in white wax, melted in an earthen pipkin smeared all over, and applied to the fire again, that the wax might be equally thick on all parts of the board, a ground was laid (on the waxed board) with levigated chalk mixed with gum water (*viz.* Gum Arabick dissolved in water): When it was dry, I painted it with a kind of landskip, and pursuing the method laid down by Count Caylus, brought it gradually to the fire; I fixed the picture on a fire screen which would preserve the heat, and communicate it to the back part of the board, this was placed first at the distance of three feet from the fire, and brought forward by slow degrees till it came within one foot of the fire, which made the wax swell and bloat up the picture; but as the chalk did not absorb the wax, the picture fell from the board and left it quite bare.

EXPERIMENT III.

I mixed three parts white wax, and one part white refin, hoping the tenacity of the refin might preserve the picture. This was laid on a board heated, with a brush, as in the former; and the ground was chalk, prepared as before. This was placed horizontally on an ironing box, charged with an hot heater, shifting it from time to time, that the wax and refin might penetrate the chalk; and hoping from this position, that the ground, bloated by melting the wax, would subside into its proper place: but this, like the other, came from the board, and would not at all adhere.

EXPERIMENT IV.

Prepared chalk four drams, white wax, white refin, of each a dram, burnt alabaster half a dram, were all powdered together and sifted, mixed with spirit of molosses instead of water, and put for a ground on a board smeared with wax and refin, as in *Exp. 3.* This was also placed horizontally on a box-iron, as the former: the picture blistered, and was cracked all over; and tho' removed from the box-iron to an oven moderately heated (in the same horizontal position), it would not subside, nor become smooth. When it was cold, I took an iron spatula made warm, and moved it gently over the surface of the picture, as if I were to spread a plaister. (This thought occurred, from the board being prepared with wax and refin, and the ground having the same materials in its composition, the force of the
spatula

spatula might make them unite.) This succeeded so well, as to reduce the surface to a tolerable degree of smoothness: but as the ground was broke off in many places, I repaired it with flake white, mixed up with the yolk of an egg and milk, and repainted it with molosses spirit (instead of water); and then put ~~it~~ into an oven with a moderate degree of heat. In this I found the colours fixed, but darker than when it was first painted; and it would bear being washed with water, not rubbed with a wet cloth.

EXPERIMENT V.

A board (that had been used in a former experiment) was smeared with wax and resin, of each equal parts; was wetted with molosses spirit, to make whitening (or Spanish white) mixed with gum-water adhere. This, when dry, was scraped with a knife, to make it equally thick in all places. It was put into a warm oven, to make the varnish incorporate partly with the whitening before it was painted; and it had only a small degree of heat: water only was used to mix the colours. This was again put into an oven with a greater degree of heat; but it flaked off from the board: whether it might be owing to the board's having had a second coat of varnish (the first having been scraped and melted off), and that the unctuous parts of the wax had so entered its pores, that it would not retain a second varnish, I cannot tell.

EXPERIMENT VI.

Having miscarried in these trials, I took a new board, planed smooth, but not polished, either ~~with~~
a fish-

a fish-skin or rushes: I warmed it, and smeared it with wax only; then took *cimolia* (tobacco-pipe clay) divested of its sand, by being dissolved in water and poured off, leaving the coarse heavy parts behind. After this was dried and powdered, I mixed it with a small quantity of the yolk of an egg and cow's milk, and made a ground with this on ~~the~~ waxed board: this I was induced to try, by knowing that the yolk of an egg will dissolve almost all unctuous substances, and make them incorporate with water; and I apprehended, that a ground, thus prepared, would adhere so much the more firmly to the board than the former had done, as to prevent its flaking off. The milk, I thought, might answer two purposes; first, uniting the ground with the wax; and secondly, by answering the end of size, or gum-water, and prevent the colours sinking too deep into the ground, or running one into another. When the ground was near dry, I smoothed it with a pallet-knife, and washed with milk and egg where I had occasion to make it smooth and even: when dry I painted it, mixing the colours with common water; this, on being placed horizontally in an oven, only warm enough to melt the wax, flaked from the board; but held so much better together than any of the former, that I pasted part of it on paper.

EXPERIMENT VII.

* Flake white mixed with egg and milk, crumbled all to pieces in the oven, put on the waxed board, as in the last experiment.

The bad success which had attended all the former experiments, led me to consider of what use the wax was in this kind of painting: and it occurred to me, that it was only as a varnish to preserve the colours from fading.

In order to try this,

EXPERIMENT VIII.

I took what the bricklayers call fine stuff, or putty †; to this I added a small quantity of burnt alabaster, to make it dry: this it soon did in the open air; but before I put on any colours, I dried it gently by the fire, lest the colours should run. When it was painted, I warmed it gradually by the fire (to prevent the ground from cracking), till it was very hot. I then took white wax three parts, white resin one part, melted them in an earthen pipkin, and with a brush spread them all over the painted board, and kept it close to the fire in a perpendicular situation, that what wax and resin the plaister would not absorb might drop off. When it was cold, I found the colours were not altered, either from the

* Flake white is the purest sort of white lead.

† Putty is lime slacked, and, while warm, dissolved in water, and strained through a sieve.

heat of the fire, or passing the brush over them. I then rubbed it with a soft linen cloth, and thereby procured a kind of gloss, which I afterwards increased by rubbing it with an hard brush; which was so far from scratching or leaving any marks on the picture, that it became more smooth and polished by it.

After I had made all the foregoing experiments, in conversation with my honoured and learned friend Dr. Kidby, a fellow of this Society, I said I had been trying to find out what the encaustic painting of the ancients was. Upon which, he told me, that there was a passage in Vitruvius *de architectura* relative to that kind of painting; and was so good as to transcribe it for me from the 7th book, chap. 9. *De mini temperatura*. Vitruvius's words are; *At si quis subtilior fuerit, & voluerit expolitionem miniacem suum colorem retinere, cum paries expolitus & aridus fuerit, tunc ceram punicam liquefactam igni, paulo oleo temperatam, seta inducat, deinde postea carbonibus in ferreo vase compositis, eam ceram apprime cum pariete, calefaciendo sudare cogat, fiatque ut peræquetur, deinde cum candela linteisque puris subigat, uti signa marmorea nuda curantur. Hæc autem xavovis græce dicitur. Ita obstant ceræ punicæ lorica non patitur, nec lunæ splendorem, nec solis radios lambendo eripere ex his politionibus colorem.*

Which I thus translate: " But if any one is more
 " wary, and would have the polishing [painting]
 " with vermilion hold its colour, when the wall is
 " painted and dry, let him take Carthaginian [Bar-
 " bary] wax, melted with a little oil, and rub it on
 " the wall with an hair pencil; and afterwards let
 " him

“ him put live coals into an iron vessel (chafing-dish),
 “ and hold it close to the wax, when the wall, by
 “ being heated, begins to sweat; then let it be made
 “ smooth: afterwards let him rub it with a * candle
 “ and † clean linen rags, in the same manner as they
 “ do the naked marble statues. This the Greeks call
 “ *καυσις*. The coat of Carthaginian wax (thus put
 “ on) is so strong, that it neither suffers the moon
 “ by night, nor the sun-beams by day, to destroy
 “ the colour.”

Being satisfied, from this passage in Vitruvius, that the manner of using wax in *Exp.* 8. was right, I was now to find if the wax-varnish, thus burnt into the picture, would bear washing: but here I was a little disappointed; for rubbing one corner with a wet linen cloth, some of the colour came off; but washing with a soft hair pencil dipped in water, and letting it dry without wiping, the colours stood very well.

A board painted, as in *Exp.* 8, was hung in the most smoaky part of a chimney for a day, and ex-

* This account of the method of polishing [painting] walls coloured with vermilion, gave me great satisfaction, as it proved the method I had taken in experiment 8. (which I had tried before I saw or knew of this passage in Vitruvius) was right. The use of the candle, as I apprehend, was to melt the wax on the walls, where by accident the brush had put on too much, or afford wax where the brush had not put on enough, or had left any part bare.

† The rubbing the wall with a linen cloth, while warm, will do very well, where there is only one colour to be preserved; but where there are many, as in a landscape, it will be apt to take off some, or render the colouring rather faint; which I found by wiping the wax off from a painting while it was hot.

posed to the open air in a very foggy night. In the morning the board was seemingly wet through, and the water ran off the picture. This was suffered to dry without wiping; and the picture had not suffered at all from the smoke or the dew, either in the ground or the colours: but when dry, by rubbing it, first with a soft cloth, and afterwards with a brush, ~~it~~ recovered its former gloss.

Suspecting that some tallow might have been mixed with the white wax I had used, which might cause the colours to come off on being rubbed with a wet cloth, I took yellow wax which had been melted from the honeycomb in a private family, and consequently not at all adulterated; to three parts of this I added one part resin, and melted them together.

EXPERIMENT IX.

Spanish white, mixed with fish glew, was put for a ground on a board, and painted with water colours only. The board was made warm; and then the wax and resin were put on with a brush, and kept close to the fire till the picture had imbibed all the varnish, and looked dry. When it was cold, I rubbed it first with a linen cloth, and then polished it with an hard brush.

In these experiments I found great difficulties with regard to colours; many water colours being made from the juices of plants, have some degree of an acid in them; and these, when painted on an alkaline ground, as chalk, whitening, *cimolia*, and plaster, are, totally changed their colours, and from green became

became brown; which contributed much to make the experiments tedious. I would therefore advise the use of mineral or metallic colours for this sort of painting, as most likely to preserve their colour: for although I neutralized Spanish white, by fermenting it with vinegar, and afterwards washed it very well with water, it did not succeed to my wish.

These experiments, and this passage from Vitruvius, will in some measure explain the obscurity of part of that passage in Pliny which Dr. Parsons, in his learned comment on the encaustic painting with wax, seems to despair of.

Ceris pingere was one species of encaustic painting. *Εγκαυστικόν, inustum*, may be translated, forced in by the means of fire, burnt in: for whatever is forced in by the help of fire can be rendered into Latin by no other significant word, that I know of, but *inustum*. If this is allowed me, and I think I have the authority of Vitruvius (a writer in the Augustan age) for it, who seems to have wrote from his own knowledge, and not like Pliny, who copied from others much more than he knew himself, the difficulty with regard to this kind of painting is solved, and the encaustic with burnt wax recovered to the public.

What he means by the next kind he mentions, *in ebore cestro id est viricali*, I will not attempt to explain at present.

The ship painting is more easily accounted for: the practice being, in part, continued to this time; and is what is corruptly called breaming, for brenning or burning. This is done by reeds set on fire, and held under the side of a ship till it is quite hot;

then resin, tallow, tar, and brimstone, melted together, and put on with an hair brush while the planks remain hot, make such a kind of paint as Pliny describes; which, he says, *nec sole, nec sale, ventisque corrumpitur*, as they were ignorant of the use of oil painting, they mixed that colour with the wax, &c. which they intended for each particular part of the ship, and put it on in the manner above described.

In the pictures painted for these experiments, and now laid before your Lordship and the Society, I hope neither the design of the landscape, nor the execution of it, will be so much taken into consideration as the varnish (which was the thing wanted in this inquiry): and I think that will evince, that the encaustic painting with burnt wax is fully restored by these experiments; and though not a new invention, yet having been lost for so many ages, and now applied further, and to other purposes than it was by Vitruvius (who confined it to vermilion only), may almost amount to a new discovery, the use of it may be a means of preserving many curious drawings to * posterity: for this kind of painting may be on paper, cloth, or any other substance that will admit a ground to be laid on it. The process is very simple, and is not attended with the disagreeable smell unavoidable in oil painting, nor with some inconveniences inseparable from that art; and as there is no substance we know, more durable than wax, it hath the greatest probability of being lasting.

* A bird painted by Mr. Edwards on paper, and the colours fixed by burned wax, was shewn to the Society, April 5th.

I ask pardon of your Lordship and the Society for taking up so much time as this paper hath required : but if it meets with your Lordship's and the Society's approbation, I may, in some future paper (when the necessary avocations of my profession will allow me leisure), lay before you some experiments, relating to colours which are not likely to change by being painted on any kind of ground.

As your Lordship's recommendation contributed much to make me a member of this learned body, I must beg your patronage of this communication ; and am, with the greatest respect,

Your Lordship's and the Society's

Most obedient humble Servant,

Budge-row,
February 27, 1759.

Josiah Colebrooke.

IX *A Letter concerning the success of the preceding Experiments. In a Letter to the Right Honourable Lord Charles Cavendish, V. P. R. S. from Mr. Josiah Colebrooke, F. R. S.*

My Lord,

Read April 5th 1759. **I**N a paper (I lately had the honour to lay before the Royal Society, on the encaustic painting of the ancients) I mentioned an use which might be made of it to preserve drawings. I have now the pleasure of laying before your Lordship

ship and the Society a specimen of the encaustic upon paper, being a bird drawn by Mr. George Edwards, a fellow of this Society, on paper prepared with a ground of whitening and fish-glew, painted with water colours, and then the wax, &c. burned in. This will roll up as easily as common paper, without cracking the varnish. There are also two landscapes, painted by a young lady, after the same manner, on wood. These will fully evince all I advanced in that paper.

I am, my Lord, with the greatest respect,

Your Lordship's most humble Servant,

Budge-row,
April 5. 1759.

Josiah Colebrooke.

X. An Account of a particular Species of Cocoon, or Silk-pod, from America. By the Reverend Samuel Pulletin, M. A.

Read Mar. 8, 1759. **H**AVING lately seen the aurelia of a particular species of caterpillar, I judged, from its texture and consistence, that there might be procured from it a silk not inferior to that of the common silk-worm in its quality, and in its quantity much superior. I have made some experiments on this new species of silk-pod, which strengthen this opinion.

This pod is about three inches and a quarter in length, and above one inch in diameter ; its outward form not
so

so regular an oval as that of the common filk-worm ; its consistence somewhat like that of a dried bladder, when not fully blown ; its colour of a reddish brown ; its whole weight 21 grains.

Upon cutting open this outer integument, there appeared in the inside a pod completely oval, as that of the filk-worm. It was covered with some floss-filk, by which it was connected to the outer coat, being of the same colour. Its length was two inches ; its diameter nearly one inch ; and its weight nine grains.

The pod could not be easily unwinded, because it was perforated by the moth : but, upon putting it in hot water, I reeled off so much as sufficed to form a judgment of the strength and staple of its filk.

The single thread winded off the pod in the same manner as that of the common filk-worm ; seeming in all respects as fine, and as tough. I doubled this thread so often as to contain twenty in thickness ; and the compound thread was as smooth, as elastic, and as glossy, as that of the common filk-worm. I tried what weight it would bear ; and it bore fifteen ounces and a half, and broke with somewhat less than sixteen, upon several trials. I then tried a thread of the common filk-worm, which was also composed of twenty (in thickness it rather exceeded the other) ; and it broke always with fifteen ounces.

I boiled a part of the *cocoon* in water, for the space of four hours, that I might know whether it was composed of a gum in any sort mucilaginous ;
and

and I found that it was as indissoluble as that of the common silk-worm.

The common silk-pod, with all its floss, weighs usually but three grains: and here is a pod which weighs seven times as much. If the outer coat, which weighed twelve grains, were all to be used only as floss-silk, there remain nine grains, capable of being reeled; which is above three times as much as can be reeled from the common *cocoon*. But I am of opinion, that when the pod is fresh, and not hardened by age, the whole outer coat may be reeled off: for the pod on which I made these trials was seven or eight years old.

Upon enquiry, I have found that the moth of this pod is called the *Isinglass* by Marian. It is a very large moth, being five inches from the tip of each wing extended. It differs from the silk-moth; in that it has a proboscis; which intimates that it feeds in its papilio state, whereas the silk-moth never eats.

The caterpillar which produces this pod is a native of America. It was found in Pennsylvania: the pod was fixed to the small branch of a tree, which seemed to be either of the crab or hawthorn species.

The leaf of the tree had also helped to support the pod; for the mark of its ribs was apparent on the surface of the pod.

I do not conceive that it will be at all difficult to find out the caterpillar, or the tree it feeds on; or to reel such a quantity of the silk as shall, when woven into ribband, more fully demonstrate whether it be of that value which I judge it. For by

comparing it with the *cocoon* of the wild Chinese silk-worm, from which an excellent species of silk is made, I have no doubt of its being the same species; and would be glad if, by this memorial, I could induce the people of America to make trial of it.

Samuel Pullett.

XI. *A Thermometrical Account of the Weather, for One Year, beginning September 1753. Kept in Maryland, by Mr. Richard Brooke, Physician and Surgeon in that Province. Communicated by Mr. Henry Baker, F. R. S.*

Read Mar. 13,
1755.

S E P T E M B E R, 1753.

A.M.]	Wind and Weather.	P.M.	Wind and Weather.
♀		♂	
79	Clear. N.W. moderate	79	Clear. S. E.
81	Flying clouds. S. E.	87	D° D°
72	D° N. W.	80	D° N.W.
70	D° S. E.	76	D° S. E.
70	D° S. W.	77	Rained. S. W.
69	Clear and calm	72	Clear and calm
83	D° D°	87	D° S.
74	Cloudy. N. E.	82	Cloudy. N. E.
9	D° Calm	79	Wind high. S. Rained
10	D° D°	75	Hard rain. Evening clear
11	D° D°	65	Flying clouds. N. E.
12	64 D° D°		<i>Intermittent fever and flux</i>
	D° S. E.	68	D° D°
23	70 Fl. clouds. N.W. brisk gal		Flying clouds. N.W. D°
24	60 Rained. N. E.	63	Rained. N. E. D°
25	62 Fair and calm		Fair
26	61 D° D°	71	Wind. S. E. D°
27	67 Flying clouds. Calm	73	Fl. clouds and calm. D°
28	66 D° N. E.	67	Calm. Rained. D°
29	63 D° D°	72	Flying clouds. N. E. D°
30	58 1 Clear and calm	73	Clear and calm. D°

OCTOBER.

O C T O B E R.

A.M.		Wind and Weather.	P.M.		Wind and Weather.
	♀			♀	
1	72	Foggy and calm	73		Misty
2	64	Cloudy. D°	66		Rained. S.
3	66	Flying clouds. S.	74		Flying clouds. S.
4	70	Thunder and rain. S.	71		Thunder and lightening
5	64	Flying clouds. N.W.	74		Flying clouds. N.W.
<hr/>					
7	56	Cloudy. D°	64		Cloudy. D°
8	54	Rained at times. D°	62		D° Calm
9	52	Cloudy. D°	56		D° D°
10	52	Rained. D°	64		Clear and D°
11	56	Flying clouds. S.			<i>Worms in children</i>
<hr/>					
14	62	Brisk gale. N.W.	68		Brisk gale. N.W. <i>more common than usual,</i>
<hr/>					
17	61	Cloudy. Calm			
18	64	Flying clouds. S. E.	65		Clear. S. <i>from infancy to</i>
19	66	D° S.			<i>the age of</i>
20	62	Cloudy and calm	71		Sunshine. S. E. <i>fourteen or fifteen</i>
21	66	Rained. E.			
<hr/>					
23	57	Flying clouds. N. E.			
<hr/>					
27	58	Brisk gale. N. W.	59		Brisk gale. N. W.
28	47	Flying clouds. D°	49		Flying clouds D° D°
29	44	Frost. D°	52		Cloudy. S. E.
30	42	Rained. Snowed. N.	36		Snowed. N. W.
31	47	Clear. High. N. W.	51		Clear. High. D° D°

N O V E M B E R.

A. M.		Wind and Weather.	P. M.		Wind and Weather.
1		Clear and calm. N. W.	61		Clear. E.
2	58	Flying clouds. S. E.	63		Rained. Brisk gale. S. E.
3		Clear. High. N. W.	39		Clear. D° N. W.
4	41	D° D° D°			D° D°
5	47	L° Calm			D° Calm. Worm fevers
<hr/>					
7	50	D° D°	61		D° S. E.
8	57	Cloudy. E.	61		Cloudy. E.
9	58	Rained. F.	62		Rained. F.
10	60	D° N. W.	62		Clear. N. W.
11	55	Fair. D°	60		D° D°
<hr/>					
13	47	Cloudy and calm	52		Rained. E.
14	43	Rain'd. High at times. S. W.	38		D° and snowed. S. W.
15	50	Clear. D°	42		Clear. D°
16	40	D° D°	41		D° D°
17	39	Cloudy. D°	41		Cloudy. Calm
18	40	Clear and calm	42		Clear. D°
19	44	D° D°	50		D° D°
20	45	Cloudy. E.	51		Rained. E.
21	50	Fair. Calm.	53		Fair. Calm.
22	44	Flying clouds. High. N. W.	46		D° High. N. W.
23	42	Snowed. L°	45		D° D°
24	43	Thin clouds. S. E.	46		Thin clouds. E.
25	46	Rained. D°	48		Rained. E.
26	38	Clear. N. W.	44		Clear. N. W.
27	44	D° Calm	45		Cloudy. D°
28	53	D° S.	58		Clear. S.
29	48	D° N. W.	50		D° N. W.
30	52	D° S. W.	56		Fair and smoky. S. W.

D E C E M -

D E C E M B E R.

A.M.		Wind and Weather.	P.	A.		Wind and Weather.
	55	Smoky and calm		5		Fair and calm.
1	54	Rained.	E.	7		Rained. E.
2	67	Cloudy and calm		9		Flying clouds
3	60	Foggy.	S.	4		D° S.
4	54	Cloudy. Rain.	N.	0		Cloudy. N.E.
5	40	Rained.	N.E.	2		Rained
6	48	D° Calm		9		D°
7	49	Cloudy. D°		7		Cloudy
8	39	Fair.	N.W.	8		Fair. N.W.
9	52	D°	D°	15		D° D°
10	45	Rained.	N.E.	13		Rained. N.E.
11	42	D°		10		D°
12	47	Fair.	S.W.	15		Fair. N.W.
13						
14	40	Rained: then Sunshine and fl. clouds. High.	N.W.	39		D° High. D°
15	33	Fair. D°	D°	36		Fair. D°
16	32	Snowed		31		D°
17	32	Fair.	D°	8		D°
18	33	D°	D°	11		D°
19	30	Cloudy. Calm		9		D° N.W.
20	35	Fair.	N.W.	12		D° D°
21	35	Snowed.	D°	41		Flying clouds. D°
22	35	Fair.	D°	41		Fair. D°
23	36	Cloudy and calm		43		Rained. High. E.
24	36	Flying clouds. High.	S.W.	40		fl. clouds. High. S.W.
25	11	Fair.	N.W.	17		Fair. N.W.
26	15	Cloudy. Calm		25		Cloudy
27	26	Snowed. D°		32		Hail
28	33	Cloudy.	N.W.	32		Fair. High. N.W.
29	11	Fair.	D°	29		D° D°
30	29	D°	D°	37		D° D°
31						

J A N U -

JANUARY, 1754.

A.M.		Wind and Weather.	P.M.	Wind and Weather.
	♀		♀	
1	21	Clear and calm	27	Clear. S.
2	29	Cloudy. D°	37	Hail, rain, and snow. S.
3	38	D°	41	Flying clouds
4	36	Thin clouds. D°	41	D°
5	39	Rained. Calm.	46	Rained. E.
6	42	D° Snowed. D°	38	Clear. Brisk. N.W.
7	27	Clear. D°	48	D° Calm
8	32	D°	45	D° W.
9	51	Cloudy. Brisk.	61	Flying clouds. S.W.
10	36	Clear and calm	51	Clear. W.
11	35	Thin clouds.	58	Thin clouds. S.W.
12	57	Rained. S.W.	64	Rained. D°
13	54	Foggy. Calm.	63	Clear. N.W.
14	53	Thin clouds. W.	63	Flying clouds D°
15	49	Cloudy. E.	50	Cloudy. S.E.
16	61	Rained. High. S.	55	Clear. High. N.W.
17	39	Clear. Moderate. N.W.	46	D° Calm
18	37	D° N.E.	48	D° S.
19	37	D° S.W.	50	D° S.W.
20	49	D° Calm	62	D° S.
21	57	Cloudy. S.	42	D° High. N.W.
22	15	Clear. Moderate. N.W.	24	D° D°
23	15	D° D°	34	Cloudy. S.
24	39	Snowed. Calm.	42	D° N.
25	36	Cloudy. N.W.	43	D° N.W.
26	33	Clear. S.	52	Clear. S.
27	39	Thin clouds. D°	61	D° Calm
28	58	D° D°	60	Rained
29	44	Cloudy. E.	46	D° E.
30	42	Foggy. N.W.	52	Clear. High. N.W.
31	44	Thin clouds	46	Rained. N.E.

F E B R U A R Y.

A.M.		Wind and Weather.		P.M.		Wind and Weather.	
	♀				♀		
1	46	Drizzle.	Calm.	53	Cloudy.	S. E.	
2	48	Foggy.	D°	54	Rained.	E.	
3	45	D°		48	D°		
4	44	Cloudy.	S. E.	46	Cloudy.	N.W.	
5	33	Clear.	N.W.	45	Clear.	D°	
6	33	Cloudy and calm		47	Cloudy and calm		
7	42	Clear.	N.W.	47	Clear.	N.E.	
8	39	Rained.	E.	49	Flying clouds.	S.W.	
9	61	D° High.	N.W.	52	Stormy weather		
10	48	at 7 o'clock, 36° at 9 o'clock.			Clear. High.	N.W.	
		Clear. High.	N.W.				
11	32	Snowed, and calm		37	Snowed.	E.	
12	10	Clear. High.	N.W.	23	at 5 o'clock, 14° at 9 o'clock		
					Clear.	N.W.	
13	11	D° Moderate.	D°	23	Clear.	D°	
14	26	Cloudy; various winds		39	D°	S.	
15	36	Thin clouds.	S.W.	51	Smoky.	S.W.	
16	49	Cloudy. Brisk gale.	D°	36	Very high.	N.W.	
17	24	Clear and calm		39	Clear.	S. E.	
18	36	Hailed and rained		55	Cloudy.	S.	
19	60	Cloudy. High.	S.	45	Rained. High.	N.W.	
20	33	Clear. Moderate.	D°	46	Clear.	S.	
22	49	Cloudy.	S.	57	Cloudy.	E.	
23	57	D°	D°	57	Flying clouds.	S.	
24	47	D°	S. E.	59	Rained. Brisk gale.	N.W.	
25	47	Snowed.	N.	33	Snowed.	Westwardly	
26	33	Drizzle		35	Drizzle		
27	46	Cloudy.	E.	50	Rained. High.	N.W.	
28		D° Moderate.	N.W.	37	Clear.	D°	

M A R C H.

M A R C H.

A.M.		Wind and Weather.		P.M.	Wind and Weather.*	
	°			°		
1	27	Clear.	N.	37	Snowed.	S. E.
2	37	Snowed		45	Rained	
3	37	Clear.	S.W.	62	Cloudy.	S.
4	52	D°	D°	71	Smoky.	S.W.
5	62	Smoky. Brisk.	D°	63	Thunder. Rain.	S.
6	49	Rained. Calm.		44	Rain. Calm	
7	42	Cloudy		45	D°	N. E.
8	39	Hail, rain, and snow.	N. E.	38	D° Hail and snow	D°
9	35	D° D°	D°	34	D° D°	Eastwardly
10	35	Drizzle	N. W.	34	Snowed.	N. W.
11	34	Clear. Brisk.	D°	48	Clouds.	Westwardly
12	41	Clouds.	S. E.		Smoky	
13		Smoky.	S.	71	D°	S.
14	63	D°	S. W.	69	D°	N. E.
15	53	D°	N. E.	55	D°	S. E.
16	57	D°	S.	66	D° Rained.	N. E.
17	47	Clear.	N. E.	49	Clear.	S. W.
18	43	Clouds.	S.	60	D°	S.
19	63	Smoky.	N. W.	63	D° Brisk gale.	N. W.
20	46	Clear.	D°	44	D°	S. E.
21	46	Rain. Thunder.	E.	43	Cloudy.	N. W.
22	38	Clear.	N. W.	44	D° Thunder and lightn.	
23		Rained.	E.	56	Smoky.	S. W.
24	53	D° Thunder		54	Clouds.	N. W.
25	48	Clear.	N. W.	53	Clear.	D°
26	42	D°	D°	45	D°	N.
27	33	D°	N.	51	D°	D°
28	47	Rained.	E.	47	Thund. lightn. rain.	N. E.
29	46	Cloudy and calm		48	Cloudy and calm	
30	45	Clear. D°		59	Clear.	E.
31	44	Thin clouds.	N.	55	Cloudy.	N.

A P R I L.

A.M.		Wind and Weather.	P.M.	Wind and Weather.
	8		8	
1	44	Clear. Various winds,	59	Clear. E.
2	45	D° E.	65	D° D°
3	47	D° S.	63	D° D°
4	51	Smoky. S.	56	Cloudy. D°
5	55	Rained. E.	53	Rained. N. E.
6	52	D° N. E.	53	Cloudy. D°
7	50	Clear. D°	56	Rained. D°
8	52	Cloudy. D°	54	Clear. D°
9	53	Drizzle. D°	56	Clouds. D°
10	57	Cloudy. S. E.	62	Rained. S.
11	57	Flying clouds. N. W.	51	Fl. clds. Brisk. N. W. Frost
12	42	Clear. Brisk. D°	47	Clear. D° D° D°
13	47	D° D° D°	50	D° moderate. D° D°
14	48	D° Calm	59	Cloudy. E.
15	54	Clouds. E.	56	Rained. D°
16	54	Rained. D°	52	Clouds. N. E.
17	44	Clear. N. W.	45	Clear. Brisk. N. W.
18	48	D° D°	53	Clouds. High. D° Frost
19	42	D° D°	50	Clear. Brisk. D°
20	46	Cloudy. N. E.	44	Rain. Snow. D°
21	44	Clear. N. W.	58	Clear. Brisk. D°
22	45	Smoky. D°	65	Smoky. D°
23	46	D° D°	70	D° S.
24	48	D° D°	59	D° D°
26	58	Rain. Thunder. D°	58	Rained. N. W.
27	54	Cloudy. D°	60	Clear. S.
28	52	Clear. D°	59	Flying clouds. Eastw. Frost
29	48	D° D°	53	Smoky. S. W.
30	54	Smoky. S. W.	73	D° D°

M A Y.

A.M.		Wind and Weather.		P.M.		Wind and Weather.	
	°				°		
1	66	Cloudy.	S.W.	66	Rained.	N.W.	
2	54	Drizzle	N.W.	54	D°	N.E.	
3	46	Cloudy	Eastwardly	53	Drizzle		
4	50	Drizzle		56	Cloudy.	S.W.	
5	56	Clear.	N.W.	67	Clear. Brisk gale.	N.W.	
6	55	Clouds.	S.W.	75	D° D°	D°	
7	72	D° Brisk gale.	D°	76	D° D°	D°	
8	60	D°	N.W.	67	Cloudy.	D°	
9	62	D°	E.	67	D°	E.	
11	65	Rained.	D°	63	Rained.	N.E.	
12	59	D°	D°	63	Clouds. Thunder.	E.	
13	60	Drizzle		68	D° D°	D°	
14	64	D° Thunder.	E.	67	Rained. D°	D°	
15	65	Rained. D°	D°	69	D° D°	D°	
16	64	Clouds. D°	D°	72	D° D°	S.E.	
17	68	Rain. D°	D°	66	D°	D°	
18	56	Clear.	N.W.	67	Fair.	N.W.	
19	61	D°	S.E.	73	Clouds.	S.E.	
20	65	Clouds.	D°	80	D°	D°	
21	72	D°	D°	83	D° Lightning	D°	
22	73	Clear.	N.W.	82	Clear.	N.W.	
23	70	D° Brisk gale.	D°	84	D° Brisk gale.	D°	
24	73	D° D°	D°	85	at 3 o'clock, 80° at 8 o'clock th. light. rain. High N.W.		
25	71	Clouds.	D°	74	Thund. Rain.	E.	
26	70	D°	E.	72	D° D° Lightning		
27	67	Cloudy		70	Rained. Thunder.	N.E.	
28	63	Drizzle.	N.E.	52	D° D°	N.W.	
29	45	Clear.	N.W.	60	Clear. Brisk gale.	D°	
30	52	Clouds. Brisk gale.	D°	63	D° D°	D°	
31	51	Clear. D°	D°	71	D° D°	D°	

[67]

J U N E.

A.M.		Wind and Weather.		P.M.		Wind and Weather.	
	°				°		
1	63	Clouds.	Brisk gale. S. E.	76	Clouds.		S. E.
2	70	D°	D°	70	Rain. Thunder.		
3	60	Clear.	S.W.	71	D° D° Lightn.		S. E.
4	67	Cloudy.	D°	70	D° D° D°		S.W.
5	65	Clear.	N.W.	70	Clouds.		N.W.
6	65	Clouds.	D°	69	Thund. lightn. rain.		D°
7	65	D° Brisk gale.	D°	64	Clouds. Brisk gale.		D°
8	60	D°	D°	63	Clouds.		
9	60	D° Brisk gale.	N. E.	66	D° D°		N. E.
10	62	Rained. High.	D°	61	Rain'd. High.		D°
11	57	Drizzle	D°	70	Clear. Brisk gale.		N.W.
12	62	Clear. Brisk gale.	N.W.	71	D°		D°
13	63	D°	D°	74	Rained a little.		S.
14	65	D°	D°	76	Clouds.		N.W.
15	68	Cloudy.	E.	75	D°		E.
16	64	Rained.	D°	75	Cloudy		D°
17	56	Fog.	S.	69	Clear.		S.
18	63	Clear.	S. E.	75	Clouds.		D°
19	67	D°	S.	75	D° Thunder.		D°
20	72	Drizzle		78	D°		E.
21	73	Cloudy.	S.	78	D°		S. E.
The <i>Via Lactea</i> N. & S. about 45° in the highest part from the eastern horizon.							
22	74	Cloudy.	S. E.	77	Rained.		S. E.
23	75	Rained.	D°	75	D° Thunder and lightn.		S. E.
24	69	Clear. Brisk gale.	N.W.	78	Brisk gale.		S. E.
25	69	D°	S. E.	79	Clear.		S.W.
26	74	Clouds.	D°	80	D°		S. E.
27	76	D°	S.	87	D°		D°
28	75	Drizzle	E.	87	Rain, thund. lightn.		N. E.
29	67	D°	D°	67	Drizzle		E.
30	71	Cloudy.	S. E.	73	D°		S. E.
				78	Thunder and rain		

J U L Y.

A.M.		Wind and Weather.	P.M.	Wind and Weather.
	8		2	
1	68	Clouds.	77	Clear. N. E.
2	72	D°	79	Clouds. S. E.
3	75	D°	80	Thunder, lightn. rain.
4	74	Rained.	71	Clear. N. W.
5	63	Clear.	74	D° S. W.
6	68	Rained.	72	D°
7	61	D°	76	D° N. E.
8	71	Cloudy.	65	Cloudy. Calm.
		Clear.	83	Clear. S. E.
9			82	Rain, thunder, lightning.
10	74	Cloudy.	87	Fair. S. W.
11	76	Fog	75	Rain, thund. lightn. N. E.
12		Clouds	76	Clear. N. W.
13	70	D°		D°
14	64	Cloudy.		D°
15	71	D°	84	Thund. lightn. rain. S. E.
16	74	Clear.	82	D° D° D° high. N. W.
17	74	D°	82	Fair. D° D°
18	77	Clouds.	78	Th. and rain. Northwardly
19	72	Cloudy.	74	D° D° Lightn. N. E.
20	71	Thunder and rain	78	Clouds
21	71	Clear	85	Clear
22	76	Clouds.	85	Thund. light. rain. S. E.
23	75	Cloudy.	81	D° D° D°
24	82	Drizzle.	68	Drizzle. N. E.
25	63	Fair	73	Fair. Brisk gale. D°
26	63	D°	71	D° D° D°
27	62	D°	73	Cloudy. D°
28	68	D°	79	Fair. N. W.
29	71	D°	70	D° S. E.
30	61	Clouds.	80	Thund. lightn. rain. E.
31	74	Rained.	73	Drizzle. D°

AUGUST.

AUGUST:

A.M.		Wind and Weather.		P.M.		Wind and Weather.	
	♀				♀		
1	64	Fair.	Brisk gale. N. E.	72	Fair:		S. E.
2	67	Cloudy.	D°	73	D°		D°
3	67	Rained.	Calm.	68	Cloudy.		D°
4	68	°	N. E.	66	Rained.		N. E.
5	63	Fair.	Calm.	73	Fair.		S. E.
6	63	°		77	D°		
7	72	D°	S. E.	79	Clouds.		S. E.
8	72	Rained		77	Cloudy		
9	73	°		73	Rained		
10	74	Rained.		78	Cloudy		
11	75	Fair.	S. E.	83	Fair.		Eastwardly
12	75	D°	Brisk gale. S. W.	80	Rained.		High. N.W.
13	74	Clouds		78	Cloudy.		Thunder. S. E.
14	72	Fair		83	at 2 o'clock, 75° at 8 o'clock		hard thunder, lightn. and rain. High wind. N. E.
15	72	Cloudy		79	Thun. lightn. & rain.		N. E.
16	75	D°		81	Cloudy.		Lightening.
17	76	Clouds		83	Thunder.		D°
18	76	D°		81	D°		D° Rain
19	75	D°		82	Cloudy		
20	70	Heavy rain		77	Thunder, lightn. rain.		E.
21	74	Fog		76	Fair		
22	76	Clear		87	Clear.		S. W.
23	79	D°		80	D°		Westwardly
24	80	D°		80	D°		D°
25	75	D°		88	D°		E.
26	79	D°		80	D°		
27	75	D°	N. N. W.	85	D°		
29	70	Cloudy		83	D°		
30	75	D°		87	D°		
31	78	Clear			D°		

XII. *A Thermometrical Account of the Weather, for Three Years, beginning September 1754. as observed in Maryland. By Mr. Richard Brooke. Communicated by Mr. H. Baker, F. R. S.*

The highest and lowest State of the Mercury in each Month is here given, and other Observations are occasionally intermixed.

[Read March 8, 1759.]

	high	low	1754.
Sept.	50	30	Winds for the most part easterly, and very rainy.
Oct.	80	34	Winds easterly, and much rain in the beginning: latter end fair.
Nov.	67	23	Winds variable. Snow and rain.
Dec.	60		Much rain.

1755.

Jan.	69	23	Much rain. Snow on the 1st day.
Feb.	64	14	Much snow.
Mar.		24	Much rain.
Apr.	40		On the 16th it snowed as hard as ever I knew. Cleared up at two o'clock P. M. all dissolved before night. Not one shower of rain this month. Wind easterly till the 14th; afterwards mostly westward.

May

	Mercury.		
	Highest	Lowest.	
May	57	47	Extremely dry : feldom any clouds : no rain. Every vegetable almost burnt up : strawberry-leaves, green plantain, and others, so crisp as to crumble. In this month many black cattle died for want of food.
June	90	70	Seasonable weather.
July	93	60	Very dry.
Aug.	90	61	Very dry.
Sept.	93	45	Very dry.
Oct.	75	36	Seasonable weather towards the end of this month. This was the driest summer and autumn ever remembered. Many springs dried up, that ran brisk before. My spring, a remarkable good one, ran very slow, and the water was unpleasant.
Nov.	65	29	On Tuesday the 18th I felt three shocks of an earthquake about eight minutes before four in the morning. The first was severest : it shook the house very much, and waked me. The second was less, and the third least of all. They succeeded each other at about one minute's distance, and were felt all over the continent.
Dec.	71	15	On the 16th there was a brisk southerly wind ; the mercury about noon at 71°. At four P.M. & at 69° : at five o'clock the wind came about to N.W. blew excessively hard, and did great damage in

in the country. A prodigious quantity of rain fell: it cleared up at six o'clock; but the wind continued blowing hard all night. At eight o'clock the \varnothing was at 43° at seven next morning at 26° , at nine at $24^{\circ}\frac{1}{2}$, and the morning following, *viz.* the 18th, the mercury was at 15° .

1756.

Jan.	73	15
Feb.	70	27
Mar.	—	—
Apr.	83	29
May	81	48
June	86	44

Not observed, being hindered by business.

Seasonable weather.

Seasonable weather.

Plenty of rain: the season much colder than usual, \varnothing standing frequently between 60° and 70° . On the 22d in the morning a black cloud came from the northward, soon overspread the hemisphere, and threatened much wind and rain; but soon blew over without much wind or rain. The sun shone clear, and the weather calm, till towards noon, when clouds collected towards the north and north-west. About three P. M. there was the most threatening appearance I ever beheld: the clouds in some places of a deep green; in others, of a footy black. At 45 min. past three it began to rain and blow, attended with remarkable severe thunder; but as the
thunder

thunder stopped the clock, I cannot say how long it lasted; but suppose near half an hour; in which time the most rain fell I ever saw. The wind did incredible damage in several parts of the country. In St. Mary's county, it is said, 200 houses were blown down, and many people killed. In every county in Maryland much damage was done by this gust, which was the most general ever remembered. It was all over New York, the Jerseys, Pennsylvania, Maryland, and Virginia, and did much damage every where. How much farther it extended, either northward or southward, I have not heard.

In these two last months an epidemical spotted fever was common in the country: I have not heard it was mortal any where. Those, who had it most severe, were relieved with what Mr. Collinson calls *apocinon*. There raged at the same time an epidemical disease amongst the dogs, which destroyed great numbers in all the northern plantations. It came from thence to the eastern shore in Maryland, where it killed most of the dogs. It now rages amongst our dogs, and scarce any recover. They are first seized with a short cough, and a stoppage in the nose, so that they are obliged to breathe thro' the mouth. In four,

<i>M. reury.</i>		
Highest.	Lowest.	
July 93	69	five, or six days after, they have a large discharge thro' the nostrils of a thick fetid matter, and a plentiful serous discharge from their eyes. Now their stomach fails, or rather they are afraid to eat; for every attempt makes them cough violently, and seems to give them great pain. Some die within two days after this discharge; some live a week, or longer: these have had food forced into their stomachs: but none ever recover, that I have heard of.
Aug. 93	68	Seasonable weather, and the most plentiful appearance of corn and tobacco I ever saw. The wheat was got in last month: it is supposed there will be the most of any year since the settlement of this country.
Aug. 93	68	Very dry. The disease amongst dogs continues, tho' less violent: many have their limbs seized with a paralysis: these all recover.
Sept. 92	60	The disease continues amongst the dogs. This month I saw a tame fox very ill with this disorder. I gave him a dose of a valuable powder, with which I have done much good; and for the knowledge of which I was obliged to my worthy friend Dr. Parsons, when I was last in England. I have known this powder cure dogs; which made me give it to this fox: but he died in three

<i>Mercury.</i>		
	Highst.	Lowest.
Oct. 7.	90	29
	<p>three minutes after ; which I attribute to the punch in which I gave it. This is the hottest and driest summer ever known in Maryland. There are great crops of corn and tobacco made ; but, through the extreme dryness of the weather, the latter crops of neither will come to perfection. Many springs are dried up, that were ever current before. Putrid bilious fevers are now very frequent in the country, and have raged for these six weeks past. So likewise has an hepatic dysentery, which, I have been informed (for it has not come within my own practice), has oftentimes been so malignant, as not to yield to any medicines that could be thought of.</p> <p>The weather seasonable. The dysenteries, that have been so fatal in many parts of the province, have reached my neighbourhood. The bilious fever now is, and for some time past has been, very common in this country. As the patients under my care had frequent large bilious stools (after the stimulus on the intestines was removed by opiates, and they voided no blood), imagining the fever and the flux owing to the same cause, only affecting different parts, I gave Dr. Parsons's powders, which I always used with success, and my patients</p>	

Mercury.		
Highest.	Lowest.	
		got well. The method I found successful, after trying many others, was to give opiates, at proper intervals, till the purging and bloody stools ceased : then four or five doses of Dr. Parsons's powders : afterwards a few doses of astringents, which never failed of curing. The opiate I used was <i>pil. matthei</i> ; which I supposed best in this putrid bilious complaint, because of the soap in its composition. It is remarkable, that the frequent repetition of the strongest astringents never decreased the quantity or number of the stools, till Dr. Parsons's powders were given. These valuable powders, which that ingenious and benevolent gentleman communicated to me, when I was at London, have been of incredible service in the plantations. I am persuaded he will readily inform any practitioner what they are ; and that nothing is wanting to bring them into general use, in all bilious and putrid fevers, but a thorough knowledge of their extraordinary efficacy.
Nov. 73	27	The hepatic flux spreads very much in my neighbourhood. On the 22d I was called to a family, where the mistress and maid were both down with this disorder, and appeared to have it very much alike. They were much griped, and purged more than

Mercury.			
	Highest.	Lowest.	
			20 times in 24 hours: sometimes blood in a large proportion was mixed with their stools; but the most troublesome symptom was a violent <i>teneismus</i> . The mistress took two doses of Dr. Parsons's powder, and the maid as many of Dr. James's. At night the mistress took an anodyne draught, and the maid <i>gr. 8 pilulæ matthæi</i> . The next day they took some astringent powders, at night their anodyne, and the day following both were well. The small-pox and swine-pox are now about the country.
Dec.	63	13	People in general very healthy. The small-pox spreads but slowly, and is very favourable.
1757.			
Jan.	65	10	Many sudden alterations, as to heat and cold, have been in this month: but the most remarkable I have ever observed, was the last day of this month, when the φ was up at 65° , and the next day, Feb. 1. when it was down at 28° , about the same hour in the day. The people of Maryland were afflicted last year with scarce any complaints which were not attended more or less with bilious symptoms; and as the year advanced, the weather being unusually hot, such symptoms grew more:

more violent, and the bile more putrescent. A bare state of the many odd cases that came under my care would fill several sheets of paper ; but as my own was very particular, I shall only mention that. I was taken on the 24th Dec. last with a slight fever and sore throat, which continued three days : then the fever and sore throat left me ; but for three nights afterwards I felt more uneasiness than I ever before was sensible of, tho' quite free from pain or fever. I waked, I suppose, at least an hundred times each of these nights. If I tried to lie on either side, I always turned insensibly on my back. When asleep, my imagination was filled with the most frightful ideas that ever disturbed an human mind ; but I could remember none of them when I awoke. I constantly lost my breath when asleep ; and was waked by an hideous whooping noise, like that of a child in a chin-cough. I presently fell asleep again, and the same horrid scene was re-acted. The seventh night from my first attack was attended with a new train of symptoms. About three o' clock in the morning I was afflicted with a most excruciating pain about the fifth or sixth vertebra of the back, like the boring of an auger. I was forced to quit the bed, and, to my great,

Mercury.		
	Highest.	Lowest.
Feb. 67	5	<p>great surprize, was easy almost as soon as I got on my cloaths: but it returned as soon as I attempted to lie down again. This lasted 27 days, ever free from pain while up, and always taken about seven hours after lying down, either by night or day. Neither the Indian bath, blisters, or any thing I could think of, did relieve me: but getting up always eased me. It was plainly a spasm of the muscle trapezeus; for if I rubbed my back ever so slightly, the pain would instantly remove to the muscles of the sternum, <i>et vice versa</i>; yet I could never feel it in my side. Whether it was leaving me or not, I cannot say: but I seemed to find great relief by drinking punch, into which Goa stone had been plentifully grated. It is very strange, that during the whole time I had no fever, or lost my appetite, tho' I grew very weak.</p> <p>It rained almost every day this month: and at this time there prevailed a disorder among the Negroes, which, I believe, was peculiar to them; for I never heard of any white person's having it. They were taken with pains in their heads, necks, shoulders, breasts, or bellies: it seldom continued long in a place, till it got to the thigh, where the complaint would form</p>

{Mercury. }

Eo |

form into a very hard and sensible tumour, generally in the triceps muscle. Emollient plaisters would commonly remove the tumour in two days time. The morbid matter would give exquisite pain in its descent down the thigh, and would collect again into a tumour, either in the ham, or in the calf of the leg. The same cataplasms continued would repel the tumour, and the patient would get well usually in about a fortnight. There was an high fever during the time. I never knew but one suppurate, tho' I have seen many in this complaint: this happened to a Negro boy about 15 years old: here the matter descended to the foot, which was very troublesome to heal.

Mar. | 65 | 30 A vast deal of rain fell this month.

Apr. | 67 | 35 The wettest and coldest April within man's memory. Impetigenous disorders very common both in Maryland and Virginia, and some very obstinate.

May | 88 | 48 Seasonable and healthy.

June | 90 | 72 An uncommon wet month. The hard rains beat off the flour, or *farina fecundans*, of the wheat; so that very little of that grain was made this year.

July | 90 | 64 Much rain. It is very remarkable, that many people, this month and the last,
in

Mercury.		
Highest.	Lowest.	
		in different parts of the country, were troubled with imposthumations under the arm, all in the <i>axilla dextra</i> . They matured, and healed up pretty easily. Whether this has any affinity to the disposition of the present and preceding year, I cannot say : I before observed, that almost every patient, in whatever disorder, had more or less of bilious symptoms. I have seen more inflammations of the neck of the bladder this month than all my life before : They were cured by antiphlogistic medicines.
• Aug.	90	67 Much rain, and thick foggy weather.
Sept.	88	47 Very wet. Within these two months I have seen five persons, and have heard of many more, who were taken with a violent pain in the <i>os frontis</i> , on the left side. The pain soon fell into the Eye on the same side, and occasioned a dimness : but this and the pain were soon removed by an epispastic behind the ear, if applied early. I was called to a Negro wench, who had had the complaint so long, that she was totally blind of both eyes ; which appeared, as in the <i>gutta serena</i> , without any inflammation, or visible defect ; but extremely painful. In her likewise the left eye was first affected, and much the most difficult of cure. Caustics behind the ears, and vitriolic
		colly-

		Mercury.		
		Highest.	Lowest.	
Oct.	67	43		Very wet. Many horses died this month of a pestilential fever. They had the symptoms Markham describes in his chapter of Pestilential. Heng-dung infused in stale urine (which Markham recommends) was found serviceable. Many people were ruined by the loss of all their horses: but none died that had plenty of the juice of rue. It is remarkable that no horses had this distemper, but those on the salts of the different rivers. Mr. Pollard, Edmunson, a gentleman on the eastern shore of Chesapeake, told me, he lost almost every horse by this disorder at his home plantation (the water there is salt), and not one in a forest pasture about a mile off.
Nov.	65	33		The disease among horses is over. On the 10th day of this month there was as severe a gust of thunder and lightning, as is common in July or August. Several horses, cattle, &c. were killed in different parts. There were the most luminous coruscations I ever saw; the whole hemisphere as it were in a blaze.
Dec.	68	28		Very variable weather: many high winds, and much rain.

XIII. *A Letter from Mr. Benjamin Wilfon,
F. R. S. to the Rev. Tho. Birch, D. D.
Secret. R. S.*

Dear Sir,

Read Mar. 22,
1759.

THE inclosed letter contains some electrical experiments and observations, which seem to merit the attention of the Royal Society. I wish therefore you would lay it before them; and at the same time signify, that I have seen all the experiments carefully made, and that the several facts therein contained are faithfully related. I am,

S I R,

Your most obedient humble Servant,

Queen-street, London,
22 March, 1759.

B. Wilfon.

*A Letter from Edward Delaval, M. A. and
Fellow of Pembroke-Hall, Cambridge, to
Mr. Benjamin Wilfon, F. R. S. containing
some Electrical Experiments and Observa-
tions.*

S I R,

Read Mar. 22,
1759.

I Send you a few electrical experiments and observations; and desire your opinion, how well they establish a convertibility, I believe hitherto unnoticed in many substances, from conductors into non-conductors of the electric fluid.

M 2

I have

I have filled several small glass tubes with the dry powders of calcined metals, *viz.* cerufs, lead ashes, minium, calx of antimony, &c. Into each end of every tube I put a piece of iron wire, which communicated with the calx, and fastened them with wax: so that the electric fluid, not being able to escape by means of the glass, must either pass thro' the calx, or not at all. Upon hanging one of the wires, bent for the purpose, to the electrified bar, and holding the other in my hand, I observed that no electric matter did pass the calx, the snaps issuing all the while from the bar, or from that wire which was in contact with the bar*.

Animal and vegetable solids also, when reduced to ashes, and interposed in the same manner between two pieces of wire, do, I find, as effectually intercept the electric stream, as the metallic calces.

From these experiments you see, that animal, vegetable, and metallic bodies, tho' such known conductors of the electric fluid while in their intire state, are easily changed into resistors or non-conductors of it.

I was led to attempt this change from its having been observed, that dry mould would not conduct the electric fluid: and from thence I suspected, that one class of the non-conductors must owe its property to an electrical virtue that would be found to reside in the calx, or earth of the chymists, after it is divested of the unctuous inflammable matter, which

* Since I wrote this letter, I have been informed, that part of this first experiment, relating to metallic calces, has been made before by Dr. Watfon. See the note Phil. Transf. vol. xlv. p. 107.

constitutes another of the chymical principles called sulphur; in like manner as this sulphur is constantly found highly electrical in all bodies where it abounds in a solid form, *viz.* resins, wax, &c.

These experiments appear to verify my supposition: for all the above-mentioned substances, which were thus changed into non-conductors, consist either wholly, or in a great measure, of earth freed from the unctuous inflammable particles; the metals not being calcineable without a degree of heat that must dissipate all their sulphur, as is evident from their not being reducible again into their metallic form without the admixture of some unctuous matter; and the same dissipation of their sulphur must take place in the animal and vegetable substances, before they become white ashes.

I shall not at present attempt an account, why bodies consisting of either of these substances separately are electric, tho' it appears to me deducible from some doctrines of Sir Isaac Newton; but only propose a thought concerning the reason why these two principles, calx and sulphur, which are known to unite in the composition of almost all bodies, should, notwithstanding they are electric when separate from each other, be yet found non-electric when united in one body.

It must be remembred, that there is a remarkable and well-known opposition in the electrical effects of these two classes; the earthy one (as glass and stones) electrifying *plus*, and the sulphureous one *minus*. Does it not seem then a thing to be expected, in a body compounded of both, that the opposite powers of these ingredients should counterbalance
and

and destroy the effects of each other, and the body in which the positive and negative ones equally prevail, become neutral, or non-electric?

I have not scrupled to rank those known positive electrics, glass and transparent stones, under that class of bodies which consists of calx or earth; because all vitrifications must proceed from previous calcinations, and all calces may be vitrified in the focus of large burning-glasses. The transparent stones also consist of little more than pure earth, free of the least mixture of oil, if we may judge of others by the chymical resolution of crystal.

There is another process, natural and without fire, which is supposed to destroy the sulphureous substance of metals, *viz.* when they are corroded, and moulder in the open air. Accordingly, with the same apparatus in which I tried the calcinations by fire, I examined the common rust of iron, and flake-white, which is the rust of lead, and find them equally converted into non-conductors in the open air.

That this change, in metals particularly, is not owing to, or promoted by, the circumstance of mere pulverization, is evident, not only because the above-mentioned calces are equally strong electrics when formed into hard masses with a thin paste of flour and water, and afterwards dried, but most clearly because the finest filings or powders of metals conduct as readily as the intire substances do. I have glass tubes armed as above, and filled with the preparations called powder of tin, &c. which conduct as well as a wire when it is not discontinued.

But notwithstanding this change will not succeed

in metallic substances upon mere pulverization, yet it seems to follow in most other hard bodies.

Having dried a piece of Portland stone, I found it conducted perfectly well; but upon powdering, and sealing it up in one of the tubes with the wire ends, as above, it became a perfect resistor, or non-conductor, like the metallic calces.

I have tried the same experiment on a variety of other bodies, particularly gum arabic and allum; and have reason to believe it will succeed in all bodies that can be pulverized in the mortar.

These last experiments seem to confirm Sir Isaac Newton's doctrine of a *medium* surrounding all bodies, which you have applied to the solution of electric phenomena, and are very analogous to the experiments you made with a *chain*, in order to shew that the resistance to the passage of the electric fluid may be increased by increasing the number of surfaces.

Another very extraordinary means of making this change in bodies, which abound in calx or earth, is by fire: not by the intense one that calcines, but by a moderate heat; their most perfect resistance, or non-conducting property, being when their heat is just tolerable to our hands.

I have some of the same Portland stone, wrought into plates nearly as thin as window-glass, which I heat to a proper degree, and then coat on both sides with metal, in order to make the Leyden experiment. When the stone is hot enough to singe paper, it conducts as perfectly as when cold; but on cooling a little, it begins not to conduct, and affords small shocks, which gradually increase in strength for about ten minutes; at which time it is about its most perfect

fect state, and remains so near a quarter of an hour : after that time the shocks gradually decrease as the stone grows cooler, till at last they quite cease, and it returns to its conducting state again : but this state appears before the stone is quite cold.

Experiments of this kind succeed in all bodies abounding in calx or earth, as stones, dried clay, wood when rotten or burnt in the fire till the surface becomes black.

Among other substances, I tried a common tobacco-pipe, part of which near the middle I heated to a proper degree, and then applied one end of it to the electrified bar, while the other was held in the hand ; and I observed that the electric fluid passed no farther along the pipe than to the heated part.

To these changes brought about with sudden violence, I must add the universal change going on in all animal and vegetable solids, as they are growing dry. Not only their ashes resist the passage of the electric fluid, but they of themselves arrive at this state while yet hard and intire ; and that much sooner than one would imagine ; for I have bones and hard wood that perfectly resist the passage, tho' yet capable of yielding a bright flame, but scarce a visible smoke : so that besides an evaporation of their moisture, but a partial progress can have been made in the discharge of their sulphur.

I submit to your judgment, how much this convertibility may contribute to a farther knowledge of the laws of electricity. I am, Sir,

Your most humble Servant,

Old Palace-Yard,
March 15, 1759.

Edward Delaval.

XIV. *An Account of the Case of William Carey, aged Nineteen, whose Tendons and Muscles are turning into Bones. In a Letter to the Right Honourable the Lord Cadogan, F. R. S. from the Rev. William Henry, D. D. F. R. S.*

Castle-Caldwell, near Enniskillen,
March 1. 1759.

My Lord,

Read Mar. 22, 1759. **H**AVING come hither with the Earl of Shelburne, on a visit to Sir James Caldwell and his Lady, we met with a young man, whose case is of so extraordinary a nature, that we thought it might be of public utility to examine into it strictly, and transmit it to your Lordship.

A great part of his body is, within the space of two years, ossified; and the ossification is continually seizing more of the muscles.

I have in the case barely set down the facts, without any reasoning thereon. But, so far as I can conjecture, there seems first to ooze out of the joints a kind of jelly, which by degrees grows hard, fills up gradually the smaller vessels, and concretes into bone. If it goes on, I believe within a very few years the man, if he can live, will be completely ossified. Perhaps it may be of some benefit to mankind to have his case made known to the Royal Society, or to the College of Physicians. Your Lordship's judgment will determine best how proper this may be.

VOL. LI.

N

My

My Lord Shelburne, and all his family, join in
all possible respects with

Your Lordship's

Most obedient,

and most humble Servant,

William Henry.

The CASE of WILLIAM CAREY.

HE was born in an island in Lough Melvill, a large lake in the northern point of the county of Leitrim in Ireland, and has continued therein, or in the adjacent lands, ever since.

He was bred up to work as a labourer, and continued in very good health from his birth until two years ago. About that time he first felt an unusual pain in his right wrist, which in August 1757 began to swell: this obliged him to cease from his usual labour. In the space of a month more, this swelling grew into an hardness, like to a bony substance; and continually shooting on, in December reached up as far as the elbow; all the muscles continually growing into a bony substance, and dilating so, that his wrist and arm are twice as thick and broad as in the beginning. About the space of a week after the pain began in his right wrist, he was seized with the like pain and swelling in the left wrist. This has proceeded in all respects in the same manner as in his right arm. The whole substance of each arm, from the elbow down to the wrists, feels as if it were one solid bone.

The

The ossification is shooting downwards into the fingers, and upwards into the elbows ; so as already to prevent the bending of the fingers or elbow of the left arm. It has likewise shot upwards, so as to seize the great muscles of each arm between the elbows and shoulders.

The continual pain and dilatation of the arms occasioned a bursting of the skin and fleshy parts about each elbow in November 1758 ; out of which oozed a thin yellowish humour, with a little digested *pus*. Some of these breaches have healed up of themselves. One small orifice in each elbow still continues to run.

In March 1758. he was seized with the like pain and swelling in his right ancle, whence such another bony substance soon grew as in his arms. This bony substance has shot up from his ancle, both in the inward and outward side of the right leg, half way up to the knee ; and the like bony substance has, in the inward side, shot downward from the pan of the knee eight inches along the shin-bone, and is daily increasing ; so that he walks with much pain and difficulty, and after resting in his walk, grows very lame. This person is of a very thin habit of body, and in size five feet nine inches ; somewhat inclined to an hectic, tho' he has no cough.

The above-mentioned William Carey was inspected, and closely examined, as to all the above particulars, at Castle-Caldwell, in the County of Fermanagh, this 1st Day of March 1759, by us

This is exactly my case.

William Carey.

SHELBURNE.

J. A. CALDWELL.

WILL^M HENRY.

*XV. A further Account of the same Case:
in a Letter to the Right Honourable the
Lord Cadogan, F. R. S. from the Rev.
William Henry, D. D. F. R. S.*

My Lord,

Read June 14, 1759. **I** HAVE now standing by me William Carey, the young man, of the ossification of whose limbs I had the honour formerly to acquaint your Lordship: and now, in obedience to your commands, give an account of his case since that time.

I had him sent up in March last to Mercer's Hospital in this city. After examining his case, the physicians and surgeons concluded, that the only probable chance to prevent the progress of the ossification, and to remove the evil already effected, was putting him into a mercurial course. This they tried; and after some slighter mercurial medicines, they, in the latter end of April, laid him down in a salivation, thro' which he passed with safety.

This dried up the running sores at his elbows, occasioned by the bursting of the skin, thro' the ossification. Some lighter callus, which was shooting into bones, seems to be softened: in consequence of which he can move his elbows, and the joints of his fingers, with more ease; and he has a little more clearness and vivacity in his countenance: but none of the ossified parts are reduced, nor is there any appearance of their reduction; and he still continues to wear an hectic look. To reduce the ossified parts,
they

they have applied to them mercurial plaisters; the effect of which time will shew.

As he is now discharged out of the hospital, they have directed him to bathe continually in the ocean, which happens to be very convenient to his habitation; and have directed him to anoint his limbs with the soapy juice of the *quercus marina*, which lies in plenty along the shore. I shall attend to the event of this process, and send your Lordship a particular account of it.

I am, with all regard,

Your Lordship's much obliged,
and most obedient humble Servant,

Dublin,
May 24. 1759.

William Henry.

XVI. *An Account of the Comet seen in May 1759.* By J. Bevis, M. B.

Read May 3, 1759. **I** Had acquainted some of my friends, that it was my opinion a comet would hardly arise above our horizon of London Sunday April the 29th; but that probably we might see one April 30th.

Sunday was very clear; but I could not see it.

Monday, not so clear: however, I saw what I took for the comet thro' the smoke of the town, about ten, near the horizon.

Tuesday, May 1. saw it very plainly, at Mr. Short's, from nine to eleven. We compared it by
means

means of the equatorial instrument with α , β , and ϵ *corvi*; whence its right ascension, at 8 h. 45 m. mean time, comes out $159^{\circ} 55' 9''$, and its south declination $25^{\circ} 52' 14''$.

Wednesday, May 2d, observed it again at Mr. Siffon's in the Strand, with a sector of 5 feet radius, and compared it with β *corvi*; whence, at 9 h. 6 m. mean time, its right ascension $158^{\circ} 47' 37''$, and its south declination $22^{\circ} 19' 23''$. The increasing moon had now much weakened the light of the comet, so that the tail and nucleus could not be distinguished as last night.

I think I may now venture to pronounce this to be the same as the comet of 1682; and am about making out its future track. If I presume rightly, it will in a short time become in a manner stationary, but diminish very fast both in size and light; the earth and it both receding from each other almost in a right line. It is at this time about four times nearer to the earth than the sun is.

May 3. 1759.

J. Bevis.

An Account of the same Comet: By Nicolas Munckley, Esq; Communicated by Nicolas Munckley, M. D. F. R. S.

Read May 3, 1759. **T**HE first certain view I had of any appearance, which could be the expected comet, was on the evening of the 30th of April, about S. S. W. a little lower than the middle of Hydra. But I did not attempt to determine its place

place with any precision, till a second observation had made me more fully satisfied of its being the phenomenon I wished it.

- The following evening, May 1st, its place about ten o'clock (as well as I was able to fix it with no better assistance than a common globe, a quadrant, and Senex's planisphere) was, right ascension about 160 deg. declination a little more than 25 deg. S. in a part of the heavens not formed into any constellation, about 9 deg. below the star in *Crater*, mark'd in Bayer's catalogue α , and nearest to χ in *Hydra*; which last star was about 3 deg. to the east of the comet. It is a luminous appearance, very evident to the naked eye (notwithstanding the light of the moon, within two or three days of her quadrature), yet rather dim than splendid; large, but
- very ill defined. The telescope, at the same time it magnifies it, seems to render it more obscure. The nucleus appears to me to be rather surrounded with a circular haziness, than to have a tail in any particular direction, especially as seen thro' a telescope.

Hampstead,
May 1. 1759.

Nicolas Munckley.

P. S. The 2d of May I saw the comet again very distinctly with the naked eye; but being then in London, without either globe or planisphere, I did not pretend to settle its place.

The cloudiness of the evenings prevented my seeing the comet any more till the 5th and 6th of May: and on these days partly thin clouds, and partly the increasing light of the moon, rendered it much less easily discernible, both by the naked
eye

eye and in the telescope. As the same causes obscured almost all the stars near it, I had great difficulty in fixing its place on the globe. It appears however, now, evidently, to be moving contrary to the order of the signs, and more considerably northwards, *i. e.* slowly retrograde, with a decreasing south latitude.

Hampstead,
May 6. 1759.

N. M.

XVII. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries, for the Year 1757, pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. & Soc. Reg. nuper Præses, by John Wilmer, M. D. clariss. Societatis Pharmaceut. Lond. Socius, Hort. Chelseæ. Præfectus & Prælector Botanic.*

Read May 3, } 1801 **A** Cinos Syriaca, folio tenuiore,
1759. } capulis hirsutis. Mor. Hist.

1802 *Ægilops Lobelii.*

1803 *Ambrosia maritima.* C. B. 138. *Ambrosia*
quibusdam. J. B. 3. 190.

1804 *Arum Zeylanicum humile latifolium, pistillo*
purp. Miller.

1805 *Astragalus caulescens erectus pilosus, floribus*
spicatis, leguminibus subulatis pilosis, Lin.
Sp. Pl. 756.

- 1806 *Cerastium*, floribus pentandris, petalis integris,
Loefl. Desc. 26.
- 1807 *Chenopodium* foliis lanceolatis carnosis, co-
rymbis dichotamis spinosis, Lin. Sp. Plant.
221.
- 1808 *Chironia* frutescens capsulifera, Lin. Sp. Plant.
190.
- 1809 *Clethra* Linnæi *Alnifolia* Americana ferrata, flo-
ribus pentapetalis albis, in spicam dispositis,
Pluk. Alm. p. 18. tab. 115. fig. 1. Catesby,
vol. 1. p. 66. tab. 66.
- 1810 *Clinopodium* orientale origani folio, fl. mini-
mo, T. Cor.
- 1811 *Delphinium* nectariis dyphillis, labellis integris,
floribus subfolitariis, foliis compositis line-
ari-multipartitis, Hort. Upsal. *Delphinium*
elatus subincanum perenne, flor. amplis
azureis, Amman. 132.
- 1812 *Doria* Americana lato rigido folio, Boerh. Ind.
Alt. 98. Virga aurea ex Nova York, foliis
symphiti majoris hirsutis, Schol. Botan. Par.
- 1813 *Gallium* album linifolium, Barrel. obs. 99.
- 1814 *Galium* caule erecto, foliis quaternis lanceo-
latis trinerviis, Fl. Lap. 60. *Gallium* album
quadrifolium erectum, Cels. Upsal. 22.
- 1815 *Galeopsis*, five *Urtica* iners, flore luteo, J. B.
- 1816 *Genista* ramis ancipitibus articulatis, foliis ova-
to-lanceolatis, Hort. Cliff. 355.
- 1817 *Geranium* *Batrachoides* Americanum macula-
tum, floribus obsolete purpureis, Hort. Elt.
158.
- 1818 *Geranium* pedunculis subunifloris, foliis quin-
quepartitis acutis, foliolis pinnatifidis, Lin.
Sp. Pl. 685.

- 1819 *Gladiolus foliis linearibus fulcatis, caule bifloro, tubo longissimo, segmentis æqualibus*, Miller's Dict.
- 1820 *Glycyrrhiza filiquosa, vel Germanica*, C. B. P. 352.
- 1821 *Hydrangea, flor. Virgin.* 50.
- 1822 *Lamium foliis caulem ambientibus*, C. B. 231.
- 1823 *Larix orientalis, fructu rotundiori obtuso*, T. 586. *Cedrus magna five Libani conifera*, J. B. 1. 277.
- 1824 *Lotus maritima lutea filiquosa, folio pingui glabro*, Bot. Monsp.
- 1825 *Lychnis faponaria dicta folio convoluto, Gentiana concava*, Ger. 253.
- 1826 *Mandragora fructu rotundo*, C. B. 169. Off. 300.
- 1827 *Mespilus spinosa, pyri folio*, H. Leyd. *Pyrantha quibusdam*. J. B. 151.
- 1828 *Mespilus foliis lanceolatis ferratis, spinis robustioribus, floribus corymbosis*, Miller's Ic.
- 1829 *Mespilus foliis cordato-ovatis, acuminatis, marginibus acute ferratis, ramis spinosis*, Miller's Ic.
- 1830 *Mitella scapo nudo*, Hort. Cliff. 167.
- 1831 *Oenothera foliis radicalibus ovatis, caulinis lanceolatis obtusis, capsulis ovatis fulcatis*, Miller's Icons.
- 1832 *Onobrychis major, filiculis echinatis, cristatis, in spica digestis*, Mor. Hist. 2. 131.
Onobrychis foliis viciæ, fructu echinato, major, C. B. 350.
- 1833 *Orobis foliis conjugatis subsessilibus, stipulatis, dentatis*, Hort. Upsal, 220.

Lathy-



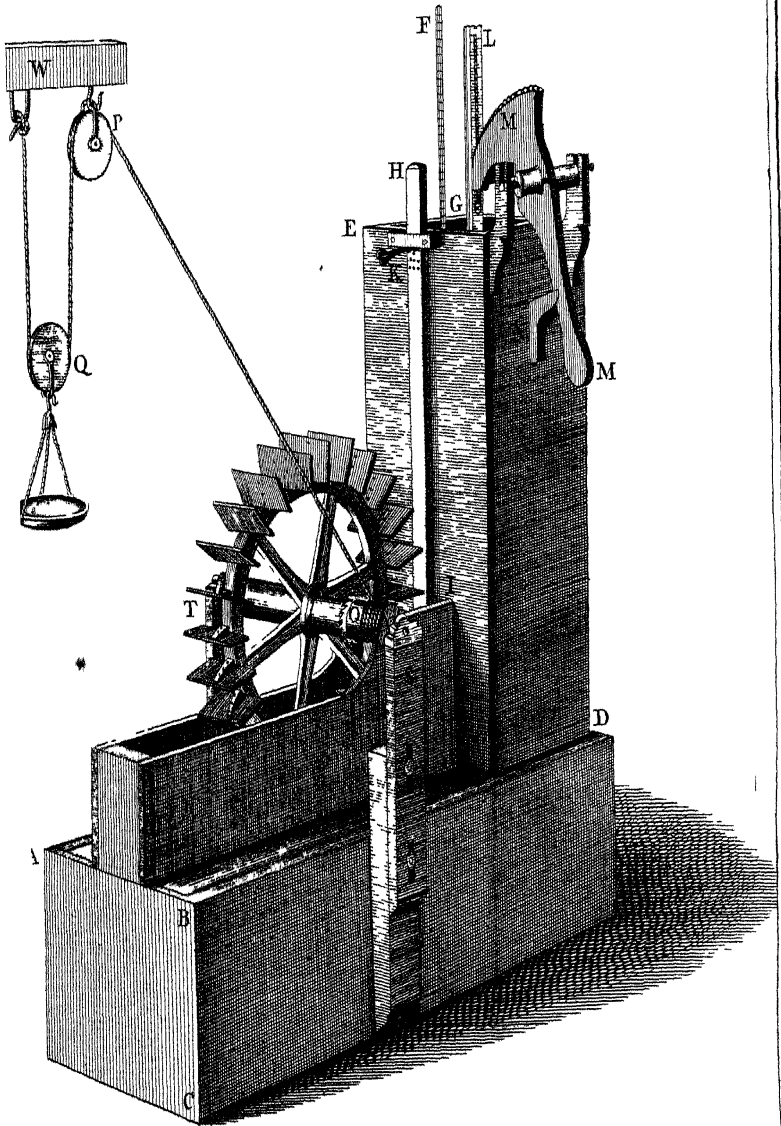
- Lathyroides erecta*, folio ovato acuminato, cæruleis vicinæ floribus, et filiquis Siberica, Amman. Ruth. 151. T. 7. F. 2.
- 1834 *Padus* foliis oblongo-ovatis semper virentibus eglandulosis, Miller's Icons.
Laurocerasus Lusitanica minor Azarero Lusitanorum, Tourn. Inst. R. H. 628.
- 1835 *Polemonium* foliis pinnatis radicibus reptatricibus, Flor. Virgin. 22.
- 1836 *Pulmonaria* calycibus abbreviatis, foliis lanceolatis obtusiusculis, Lin. Sp. Pl. 135.
Pulmonaria non maculosa foliis glabris, Americana flore patulo cæruleo, Pluk. Phyt. Tab. 227. Fig. 6.
- 1837 *Salicornia*, T. Cor. 51. *Kali geniculatum*, Ger. Em. 535.
- 1838 *Senecio* corollis radiantibus, foliis crenatis, infimis cordatis petiolatis, superioribus pinnatifidis lyratis, Flor. Virgin.
- 1839 *Serratula*, C. B. P. 235. *Jacea nemorensis*, quæ *Serratula* vulgo, T. 444.
- 1840 *Serratula* foliis oblongo-ovatis, obtuse dentatis, caule ramoso patulo, calycibus subrotundis mollibus, Miller's Dict.
- 1841 *Spartium* ramis oppositis angulatis, foliis oppositis subulatis, Lin. Sp. Pl. 708.
- 1842 *Spiræa Africana* odorata, foliis pilosis, Com. rar. 3.
- 1843 *Stramonium Americanum* maximum, flore albo, fructu rotundo spinoso, Tourn.
- 1844 *Tilia* foliis molliter hirsutis, viminibus rubris, fructu tetragono, Ray, Synop. 2. 316.
Tilia hirsuta coryli foliorum æmula, fructu anguloso, Pluk. Mant. 181.

- 1845 *Trillium flore pedunculato cernuo*, Lin. Sp. Pl. 339.
 1846 *Valeriana montana subrotundo folio*, C. B. P. 165.
 1847 *Vicia pedunculis multifloris*, petiolis polyphillis, foliolis lanceolatis glabris, Hort. Upsal, 219.
 1848 *Vicia fylvatica multiflora maxima perennis*, tetro odore, floribus albetibus, lineis cæruleis striatis, Pluk. Alm. 387.
 1849 *Vinca foliis oblongo-ovatis integerrimis*, tubo floris longissimo, caule ramofo fruticoso, Miller's Iconf.
 1850 *Xanthium*, five *Lappa minor*, J. B. 3. 572.
Lappa minor, five *Xanthium Dioscorid.* C. B. P. 193.

XVIII. *An experimental Enquiry concerning the natural Powers of Water and Wind to turn Mills, and other Machines, depending on a circular Motion. By Mr. J. Smeaton, F. R. S.*

Read May 3,
& 10, 1759.

WHAT I have to communicate on this subject was originally deduced from experiments made on working models, which I look upon as the best means of obtaining the outlines in mechanical enquiries. But in this case it is very necessary to distinguish the circumstances in which a model differs from a machine in large; otherwise a model is more apt to lead us from the truth



truth than towards it. Hence the common observation, that a thing may do very well in a model, that will not answer in large. And indeed, tho' the utmost circumspection be used in this way, the best structure of machines cannot be fully ascertained, but by making trials with them, when made of their proper size. It is for this reason, that, tho' the models referred to, and the greatest part of the following experiments, were made in the years 1752 and 1753, yet I deferred offering them to the Society, till I had an opportunity of putting the deductions made therefrom in real practice, in a variety of cases, and for various purposes; so as to be able to assure the Society, that I have found them to answer.

P A R T I.

Concerning UNDERSHOT WATER-WHEELS.

PLATE IV. Fig. 1. is a perspective view of the machine for experiments on water-wheels; wherein ABCD is the lower cistern, or magazine, for receiving the water, after it has quitted the wheel; and for supplying

DE the upper cistern, or head; wherein the water being raised to any height required, by a pump, that height is shewn by

F G, a small rod, divided into inches and parts; with a float at the bottom, to move the rod up and down, as the surface of the water rises and falls.

HI is a rod by which the sluice is drawn, and stopt at any height required, by means of

K a pin or peg, which fits several holes, placed
in

in the manner of a diagonal scale, upon the face of the rod H I.

G L is the upper part of the rod of the pump, for drawing the water out of the lower cistern, in order to raise and keep up the surface thereof at its desired height, in the head D E; thereby to supply the water, expended by the aperture of the sluice.

M M is the arch and handle for working the pump, which is limited in its stroke by

N a piece for stopping the handle from raising the piston too high; that also being prevented from going too low, by meeting the bottom of the barrel.

O is the cylinder, upon which a cord winds, and which being conducted over the pullies **P** and **Q**, raises

R, the scale, into which the weights are put, for trying the power of the water.

S T the two standards, which support the wheel, are made to slide up and down, in order to adjust the wheel, as near as possible, to the floor of the conduit.

W the beam which supports the scale and pulleys; this is represented as but little higher than the machine, for the sake of bringing the figure into a moderate compass, but in reality is placed 15 or 16 feet higher than the wheel.

PLATE V. Fig. 2. is a section of the same machine, wherein the same parts are marked with the same letters as in Fig. 1. Besides which

X X is the pump barrel, being 5 inches diameter, and 11 inches long.

Y is

Scale of inches to fig. 1^d
 0 1 2 3 4 5 6 7 8 9 10 11 12

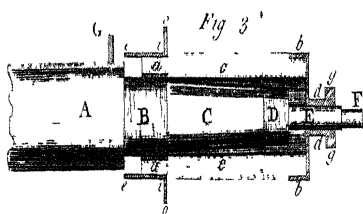
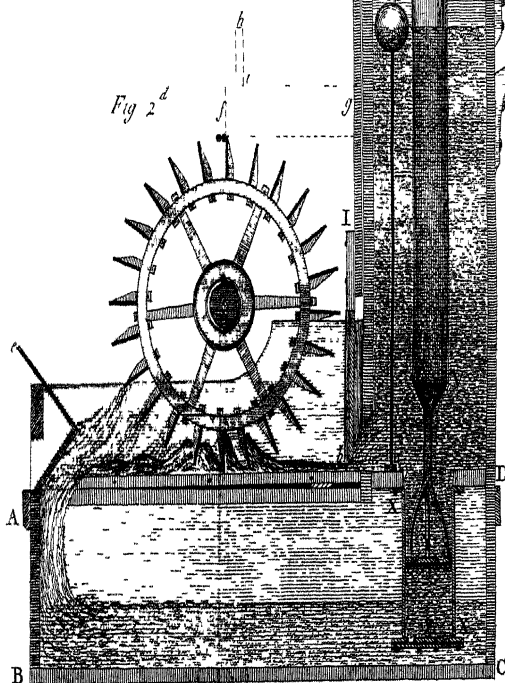


Fig 2^d



Scale of Feet to fig. 2^d
 0 1 2 3 4 5
 12 Inches

Y is the piston ; and

Z the fixed valve.

G V is a cylinder of wood, fixed upon the pump-rod, and reaches above the surface of the water ; this piece of wood being of such a thickness, that its section is half the area of that of the pump-barrel, will cause the surface of water to rise in the head, as much while the piston is descending, as while it is rising : and will thereby keep the gauge-rod F G more equally to its height. *Note*, the arch and handle M M is here represented on a different side to what it is shewn in the preceding figures, in order that its dimensions may the better appear.

a a shews one of the two wires which serve as directors to the float, in order that the gauge rod F G may be kept perpendicular ; for the same purpose also serves w, a piece of wood with a hole to receive the gauge-rod, and keep it upright.

b is the aperture of the sluice.

c c a kant-board, for throwing the water more directly down the opening c d, into the lower cistern : and

c e is a sloping board, for bringing back the water that is thrown up by the floats of the wheel.

Fig. 3. represents one end of the main axis, with a section of the moveable cylinder, marked O in the preceding figures.

A B C D is the end of the axis ; whereof the parts B and D are covered with ferrules or hoops of brass.

E is a cylinder of metal ; whereof the part marked F is

F is the pivot or gudgeon.

cc is the section of an hollow cylinder of wood, the diameter of the interior part being somewhat larger than the cylindrical ferrule B.

aa is the section of a ferrule of brass, driven into the end of the hollow cylinder, and which is adjusted to that marked B, so as to slide freely thereupon, but with as little shake as possible.

bb, *dd*, *gg*, represent the section of a brass ferrule, plate, and socket, fixed upon the other end of the hollow cylinder; the socket *dd* being adjusted to slide freely upon the cylinder E, in the same manner as the ferrule *aa* slides upon the cylinder B: the outer end of the socket at

gg is formed into a sort of button; by pushing whereof, the hollow cylinder will move backwards and forwards, or turn round at pleasure upon the cylindrical parts of the axis B and E.

ee, *ii*, *oo*, represent the section of a brass ferrule, also fixed upon the hollow cylinder: the edge of this ferrule

ee is cut into teeth, in the manner of a *contrate* wheel; and the edge thereof

oo is cut in the manner of a ratchett.

Of consequence, when the plate *b d d b* is pushed close to the ferrule D, the teeth of the ferrule *ee* will lay hold of

G, a pin fixed into the axis; by which means the hollow cylinder is made to turn along with the wheel and axis: but being drawn back by the button *gg*, the hollow cylinder is thereby disengaged from the pin G, and ceases turning.

Note. The weight in the scale is prevented from

from running back, by a catch that plays in and lays hold of the ratchet *oo*.

By this means the hollow cylinder upon which the cord winds, and raises the weight, is put in action and discharged therefrom instantaneously, while the wheel is in motion: for without some contrivance of this kind, it would not be easy to make this sort of experiments with any tolerable degree of exactness.

The use of the apparatus now described will be rendered more intelligible, by giving a general idea of what I had in view; but as I shall be obliged to make use of a term which has heretofore been the cause of disputation, I think it necessary to assign the sense in which I would be understood to use it; and in which I apprehend it is used by practical *Mechanicks*.

The word *Power*, as used in practical mechanicks, I apprehend to signify the exertion of strength, gravitation, impulse, or pressure, so as to produce motion: and by means of strength, gravitation, impulse, or pressure, compounded with motion, to be capable of producing an effect: and that no effect is properly mechanical, but what requires such a kind of power to produce it.

The raising of a weight, relative to the height to which it can be raised in a given time, is the most proper measure of power; or, in other words, if the weight raised is multiplied by the height to which it can be raised in a given time, the product is the measure of the power raising it; and consequently, all those powers are equal, whose products, made by

such multiplication, are equal: for if a power can raise twice the weight to the same height; or the same weight to twice the height, in the same time that another power can, the first power is double the second: and if a power can raise half the weight to double the height; or double the weight to half the height, in the same time that another can, those two powers are equal. But note, all this is to be understood in case of slow or equable motion of the body raised; for in quick, accelerated, or retarded motions, the *vis inertiae* of the matter moved will make a variation.

In comparing the effects produced by water-wheels, with the powers producing them; or, in other words, to know what part of the original power is necessarily lost in the application, we must previously know how much of the power is spent in overcoming the friction of the machinery, and the resistance of the air; also what is the real velocity of the water at the instant that it strikes the wheel; and the real quantity of water expended in a given time.

From the velocity of the water, at the instant that it strikes the wheel, given; the height of head productive of such velocity can be deduced, from acknowledged and experimented principles of hydrostatics: so that by multiplying the quantity, or weight of water, really expended in a given time, by the height of head so obtained; which must be considered as the height from which that weight of water had descended in that given time; we shall have a product, equal to the original power of the water; and clear of all uncertainty, that would arise from the friction of the water, in passing small apertures;
and

and from all doubts, arising from the different measure of spouting waters, assigned by different authors. On the other hand, the sum of the weights raised by the action of this water, and of the weight required to overcome the friction and resistance of the machine, multiplied by the height to which the weight can be raised in the time given, the product will be equal to the effect of that power; and the proportion of the two products will be the proportion of the *power* to the *effect*: so that by loading the wheel with different weights successively, we shall be able to determine at what particular load, and velocity of the wheel, the effect is a *maximum*.

The manner of finding the real velocity of the water, at the instant of its striking the wheel; the manner of finding the value of the friction, resistance, &c. in any given case; and the manner of finding the real expence of water, so far as concerns the following experiments, without having recourse to theory; being matters upon which the following determinations depend, it will be necessary to explain them.

To determine the Velocity of the Water striking the Wheel.

It has already been mentioned, in the references to the figures, that weights are raised by a cord winding round a cylindrical part of the axis. First, then, let the wheel be put in motion by the water, but without any weights in the scale; and let the number of turns in a minute be 60: now it is evident, that was the wheel free from friction and resistance, that 60 times the circumference of the wheel

would be the space through which the water would have moved in a minute; with that velocity wherewith it struck the wheel: but the wheel being incumbered by friction and resistance, and yet moving 60 turns in a minute, it is plain, that the velocity of the water must have been greater than 60 circumferences before it met with the wheel. Let now the cord be wound round the cylinder, but contrary to the usual way, and put a weight in the scale; the weight so disposed (which may be called the *counter-weight*) will endeavour to assist the wheel in turning the same away, as it would have been turned by the water: put therefore as much weight into the scale as, without any water, will cause it to turn somewhat faster than at the rate of 60 turns in a minute; suppose 63: let it now be tried again by the water, assisted by the weight; the wheel therefore will now make more than 60 turns; suppose 64: hence we conclude the water still exerts some power in giving motion to the wheel. Let the weight be again increased, so as to make $64\frac{1}{2}$ turns in a minute without water: let it once more be tried with water as before; and suppose it now to make the same number of turns with water as without. *viz.* $64\frac{1}{2}$: hence it is evident, that in this case the wheel makes the same number of turns in a minute, as it would do if the wheel had no friction or resistance at all; because the weight is equivalent thereto; for was it too little, the water would accelerate the wheel beyond the weight; and if too great, retard it; so that the water now becomes a *regulator* of the wheel's motion; and the velocity of its circumference becomes a measure of the velocity of the water.

In like manner, in seeking the greatest product, or *maximum* of effect; having found by trials what weight gives the greatest product, by simply multiplying the weight in the scale by the number of turns of the wheel, find what weight in the scale, when the cord is on the contrary side of the cylinder, will cause the wheel to make the same number of turns the same way, without water; it is evident that this weight will be nearly equal to all friction and resistance taken together; and consequently, that the weight *in* the scale, with twice * the weight *of* the scale, added to the back or counter-weight, will be equal to the weight that could have been raised, supposing the machine had been without friction or resistance; and which multiplied by the height to which it was raised, the product will be the greatest effect of that power.

The quantity of water expended is found thus:

The pump made use of for replenishing the head with water was so carefully made, that no water escaping back by the leathers, it delivered the same quantity of water at every stroke, whether worked quick or slow; and as the length of the stroke was limited, consequently the value of one stroke (or on account of more exactness 12 strokes) was known, by the height to which the water was thereby raised in the head; which being of a regular figure was easily measured. The sluice, by which the water was drawn upon the wheel, was made to stop at certain heights by a peg; so that when the peg was in the same hole,

* The weight of the scale makes part of the weight both ways.
the

the aperture for the effluent water was the same. Hence the quantity of water expended by any given head, and opening of the sluice, may be obtained: for by observing how many strokes a minute was sufficient to keep up the surface of the water at the given height, and multiplying the number of strokes by the value of each, the water expended by any given aperture and head in a given time will be given.

These things will be further illustrated by going over the *calculus* of one sett of experiments.

Specimen of a Sett of Experiments.

The sluice drawn to the 1 ft hole.

The water above the floor of the sluice 30 Inches.

Strokes of the pump in a minute — 39½

The head raised by 12 strokes — 21 Inches.

The wheel raised the empty scale, and made turns
in a minute ————— 80

With a counter-weight of 1 lb. 8 oz. it made 85

D^o tried with water ————— 86

N ^o	Weight. lb. oz.	Turns in a min.	Product.
1 —	4 0	— 45 —	180
2 —	5 0	— 42 —	210
3 —	6 0	— 36½ —	217½
4 —	7 0	— 33¾ —	236¾
5 —	8 0	— 30 —	240 maximum.
6 —	9 0	— 26½ —	238½
7 —	10 0	— 22 —	220
8 —	11 0	— 16½ —	181½
9 —	12	* ceased working.	

* *N. B.* When the wheel moves so slow as not to rid the water so fast as supplied by the sluice, the accumulated water falls back upon the aperture, and the wheel immediately ceases moving.

Counter

Counter-weight, for 30 turns without water, 2 oz. in the scale.

N. B. The area of the head was 105,8 square inches.

Weight of the empty scale and pulley, 10 oz.

Circumference of the cylinder, 9 inches.

Circumference of the water-wheel, 75 ditto.

Reduction of the above Set of Experiments.

The circumference of the wheel, 75 inches, multiplied by 86 turns, gives 6450 inches for the velocity of the water in a minute; $\frac{1}{60}$ of which will be the velocity in a second, equal to 107,5 inches, or 8,96 feet, which is due to a head of 15 inches *; and this we call the *virtual* or *effective* head.

The area of the head being 105,8 inches, this multiplied by the weight of water of the inch cubic, equal to the decimal ,579 of the ounce avoirdupoise, gives 61,26 ounces for the weight of as much water, as is contained in the head, upon 1 inch in depth, $\frac{1}{12}$ of which is 3,83 pounds; this multiplied by the depth 21 inches, gives 80,43 lb. for the value of 12 strokes; and by proportion, $39\frac{1}{2}$ (the number made in a minute) will give 264,7 lb. the weight of water expended in a minute.

Now as 264,7 lb. of water may be considered as having descended through a space of 15 inches in a minute, the product of these two numbers 3970 will express the *power* of the water to produce mechanical effects; which were as follows.

* This is determined upon the common maxim of hydrostatics, that the velocity of spouting waters is equal to the velocity that an heavy body would acquire in falling from the height of the reservoir; and is proved by the rising of jets to the height of their reservoirs nearly.

The velocity of the wheel at the *maximum*, as appears above, was 30 turns a minute; which multiplied by 9 inches, the circumference of the cylinder, makes 270 inches; but as the scale was hung by a pulley and double line, the weight was only raised half of this, *viz.* 135 inches.

The weight in the scale at the maximum 8 lb. 0 oz.

Weight of the scale and pulley — 0 10

Counterweight, scale, and pulley — 0 12

Sum of the resistance — — 9 6
or lb. 9,375.

Now as 9,375 lb. is raised 135 inches, these two numbers being multiplied together, the product is 1266, which expresses the effect produced at a maximum: so that the proportion of the *power* to the *effect* is as 3970 : 1266, or as 10 : 3,18.

But tho' this is the greatest *single* effect producible from the power mentioned, by the impulse of the water upon an undershot wheel; yet, as the whole power of the water is not exhausted thereby, this will not be the true ratio between the *power* of the water, and the *sum* of all the *effects* producible therefrom: for as the water must necessarily leave the wheel with a velocity equal to the wheel's circumference, it is plain that some part of the power of the water must remain after quitting the wheel.

The velocity of the wheel at the maximum is 30 turns a minute; and consequently its circumference moves at the rate of 3,123 feet a second, which answers to a head 1,82 inches; this being multiplied by the expence of water in a minute, *viz.* 264,7 lb. produces 481 for the power *remaining* in the water after it has passed the wheel: this being therefore deducted

deducted from the original power 3970, leaves 3489, which is that *part* of the power which is spent in producing the effect 1266; and consequently the part of the power spent in producing the effect, is to the greatest effect producible thereby as 3489 : 1266 : 10 : 3,62, or as 11 to 4.

The *velocity of the water* striking the wheel has been determined to be equal to 86 circumferences of the wheel per minute, and the *velocity of the wheel* at the *maximum* to be 30; the velocity of the water will therefore be to that of the wheel as 86 to 30, or as 10 to 3,5, or as 20 to 7.

The *load at the maximum* has been shown to be equal to 9 lb. 6 oz. and that the wheel ceased moving with 12 lb. in the scale: to which if the weight of the scale is added, *viz.* 10 ounces *, the proportion will be nearly as 3 to 4 between the load at the *maximum* and *that* by which the wheel is stopped.

It is somewhat remarkable, that tho' the velocity of the wheel in relation to the water turns out greater than $\frac{1}{3}$ of the velocity of the the water, yet the impulse of the water in the case of a *maximum* is more than double of what is assigned by theory; that is, instead of $\frac{4}{9}$ of the column, it is nearly equal to the whole column.

It must be remembred, therefore, that, in the present case, the wheel was not placed in an open river, where the natural current, after it has communicated its impulse to the float, has room on all sides to escape, as the theory supposes; but in a conduit or

* The resistance of the air in this case ceases, and the friction is not added, as 12 lb. in the scale was sufficient to stop the wheel after it had been in full motion; and therefore somewhat more than a counterbalance to the impulse of the water.

race, to which the float being adapted, the water cannot otherwise escape than by moving along with the wheel. It is observable, that a wheel working in this manner, as soon as the water meets the float, receiving a sudden check, it rises up against the float, like a wave against a fixed object; insomuch that when the sheet of water is not a quarter of an inch thick before it meets the float, yet this sheet will act upon the whole surface of a float, whose height is 3 inches; and consequently was the float no higher than the thickness of the sheet of water, as the theory also supposes, a great part of the force would have been lost, by the water's dashing over the float *.

In further confirmation of what is already delivered, I have adjoined the following table, containing the result of 27 sets of experiments, made and reduced in the manner above specified. What remains of the theory of undershot wheels, will naturally follow from a comparison of the different experiments together.

* Since the above was wrote, I find that Professor Euler, in the Berlin Acts for the year 1748, in a memoire intituled, *Maxims pour aranger le plus avantageusement les machines destinées à élever de l'eau par le moyen de pompes*, page 192. § 9. has the following passage; which seems to be the more remarkable, as I don't find he has given any demonstration of the principle therein contained, either from theory or experiment; or has made any use thereof in his calculations on this subject.—“ *Cependant dans ce cas puisque l'eau est réfléchie, & qu'elle decoule sur les aubes vers les cotes, elle y exerce encore une force particuliere, dont l'effet de l'impulsion sera augmenté; & experience jointe a la theorie a fait voir que dans ce cas, la force est presque double: de sorte qu'il faut prendre le double de la section du fil d'eau pour ce qui repond dans ce cas a la surface des aubes, pourvu qu'elles soient assez larges pour recevoir ce supplement de force. Car si les aubes n'étoient plus larges que le fil, on trait d'eau on ne devroit prendre que la simple section, tout comme dans le premier cas, on l'aube toute entiere est pappée par l'eau.*”

TABLE I.

No.	Height of the water in the cistern.	Turns of the wheel unloaded.	Virtual head deduced therefrom.	Turns at the maximum.	Load at the equilibrium.	Load at the maximum.	Water expended in a minute.	Power.	Effect.	Ratio of the power and effect.	Ratio of the velocity of the water and wheel.	Ratio of the load at the equilibrium, to the load at the maximum.	Experiments.
	In.		In.		lb. oz.	lb. oz.							
1	33	88	15,85	30,	13 10	10 9	275,	4358	1411	10:3,24	10:3,4	10:7,75	At the 1st hole.
2	30	86	15,0	30,	12 10	9 6	264,7	3970	1266	10:3,2	10:3,5	10:7,4	
3	27	82	13,7	28,	11 2	8 6	243,	3329	1044	10:3,15	10:3,4	10:7,5	
4	24	78	12,3	27,7	9 10	7 5	235,	2890	901,4	10:3,12	10:3,55	10:7,53	
5	21	75	11,4	25,9	8 10	6 5	214,	2439	735,7	10:3,02	10:3,45	10:7,32	
6	18	70	9,95	23,5	6 10	5 5	199,	1970	561,8	10:2,85	10:3,36	10:8,02	
7	15	65	8,54	23,4	5 2	4 4	178,5	1524	442,5	10:2,9	10:3,6	10:8,3	
8	12	60	7,29	22,	3 10	3 5	161,	1173	328	10:2,8	10:3,77	10:9,1	
9	9	52	5,47	19,	2 12	2 8	134,	733	213,7	10:2,9	10:3,65	10:9,1	
10	6	42	3,55	16,	1 12	1 10	114,	404,7	117	10:2,82	10:3,8	10:9,3	
11	24	84	14,2	30,75	13 10	10 14	342,	4890	1505	10:3,075	10:3,66	10:7,9	At the 2d.
12	21	81	13,5	29,	11 10	9 6	297,	4009	1223	10:3,01	10:3,62	10:8,05	
13	18	72	10,5	26,	9 10	7 8	285,	2993	975	10:3,25	10:3,6	10:8,75	
14	15	69	9,6	25,	7 10	6 14	277,	2659	774	10:2,92	10:3,62	10:9,	
15	12	63	8,0	25,	5 10	4 14	234,	1872	549	10:2,94	10:3,97	10:8,7	
16	9	56	6,37	23,	4 0	3 13	201,	1280	390	10:3,05	10:4,1	10:9,5	
17	6	46	4,25	21,	2 8	2 4	167,5	712	212	10:2,98	10:4,55	10:9,	
18	15	72	10,5	29,	11 10	9 6	357,	3748	1210	10:3,23	10:4,02	10:8,05	The 3d.
19	12	66	8,75	26,75	8 10	7 6	330,	2887	878	10:3,05	10:4,05	10:8,1	
20	9	58	6,8	24,5	5 8	5 0	255,	1734	541	10:3,01	10:4,22	10:9,1	
21	6	48	4,7	23,5	3 2	3 0	228,	1064	317	10:2,99	10:4,9	10:9,6	
22	12	68	9,3	27,	9 2	8 6	359,	3338	1006	10:3,02	10:3,97	10:9,17	4th.
23	9	58	6,8	26,25	6 2	5 13	332,	2257	686	10:3,04	10:4,52	10:9,5	
24	6	48	4,7	24,5	3 12	3 8	262,	1231	385	10:3,13	10:5,1	10:9,35	
25	9	60	7,29	27,3	6 12	6 6	355,	2588	783	10:3,03	10:4,55	10:9,45	5th.
26	6	50	5,03	24,6	4 6	4 1	307,	1544	450	10:2,92	10:4,9	10:9,3	
27	6	50	5,03	26,	4 15	4 9	360,	1811	534	10:2,95	10:5,2	10:9,25	6th.
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	

Maxims and Observations deduced from the foregoing Table of Experiments.

Maxim I. *That the virtual or effective head being the same, the effect will be nearly as the quantity of water expended.*

This will appear by comparing the contents of the columns 4, 8, and 10, in the foregoing sets of experiments; as for

Example 1st, taken from N^o. 8. and 25, viz.

N ^o .	Virtual Head.	Water expended.	Effect.
8	7,29	161	328
25	7,29	355	785

Now the heads being equal, if the effects are proportioned to the water expended, we shall have by maxim 1st, $161 : 355 :: 328 : 723$; but 723 falls short of 785, as it turns out in experiment according to N^o. 25, by 62; the effect therefore of N^o. 25, compared with N^o. 8, is greater than according to the present maxim in the ratio of 14 to 13.

The foregoing example, with four similar ones, are seen at one view in the following Table.

Examples	N ^o Tab. I.	Virtual Head.	Expende of Water.	Effect.	Comparifon.	Variation	Proportional Variation.
1st	8	7,29	161	328	$161 : 355 :: 328 : 723$	62+	14 : 13
	25	7,29	355	785			
2d	13	10,5	285	975	$285 : 357 :: 975 : 1221$	11—	121 : 122
	18	10,5	357	1210			
3d	22	6,8	255	541	$255 : 332 :: 541 : 704$	18—	38 : 39
	23	6,8	332	686			
4th	21	4,7	228	317	$228 : 262 :: 317 : 364$	21+	18 : 17
	24	4,7	262	385			
5th	26	5,03	307	450	$307 : 360 :: 450 : 531$	3+	178 : 177
	27	5,03	360	534			

Hence

Hence therefore, in comparing different experiments, as some fall short, and others exceed the maximum, and all agree therewith, as near as can be expected, in an affair where so many different circumstances are concerned; we may, according to the laws of reasoning by induction, conclude the maxim true; *viz.* that the effects are nearly as the quantity of water expended.

Maxim II. *That the expence of water being the same, the effect will be nearly as the height of the virtual or effective head.*

This also will appear by comparing the contents of columns 4, 8, and 10, in any of the sets of experiments.

Example 1st, of N°. 2. and N°. 24. viz.

N°.	Virt. Head.	Expence.	Effect.
2	15	264,7	1266
24	4,7	262	385

Now as the expences are not quite equal, we must proportion one of the effects accordingly: thus

by maxim 1st, $262 : 264,7 :: 385 : 389$

and by max. 2d, $15 : 4,7 :: 1266 : 397$

Difference — 8

The effect therefore of N°. 24. compared with N°. 2. is less than according to the present maxim in the ratio of 49 : 50.

The foregoing, and two other similar examples, are comprised in the following Table.

Examples

Examples.	N ^o . Tab. I.	Virtual Head.	Expende of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
1ft	2	15	264,7	1266	} Max. 1ft, 262 : 264,7 :: 385 : 319 } Max. 2d, 15 : 4,7 :: 1266 : 397	8 —	49 : 50
	24	4,7	262	385			
2d	1	15,85	275	1411	} Max. 1ft, 114 : 275 :: 117 : 282 } Max. 2d, 15,85 : 3,55 :: 1411 : 316	34 —	8 : 9
	10	3,55	114	117			
3d	11	14,2	342	1505	} Max. 1ft, 167,5 : 342 :: 212 : 433 } Max. 2d, 14,2 : 4,25 :: 1505 : 450	17 —	25 : 26
	17	4,25	167,5	212			

Maxim III. *That the quantity of water expended being the same, the effect is nearly as the square of its velocity.*

This will appear by comparing the contents of columns 3, 8, and 10, in any of the sets of experiments ; as for

Example 1st of N^o. 2. with N^o. 24. viz.

N ^o .	Turns in a min.	Expende.	Effect.
2	86	264,7	1266
24	48	262	385

The velocity being as the number of turns, we shall have,

$$\begin{aligned} &\text{by max. 1ft, } 262 : 264,7 :: 385 : 389 \\ &\text{and by max. 3d, } \left\{ \begin{array}{l} 86^2 : 48^2 \\ 7396 : 2304 \end{array} \right\} :: 1266 : 394 \\ &\text{Difference} \quad \quad \quad \underline{\quad} \quad 5 \end{aligned}$$

The effect therefore of N^o. 24. compared with N^o. 2. is less than by the present maxim in the ratio of 78 : 79.

The foregoing, and three other similar examples, are comprised in the following Table.

Examples,

Examples.	No. Tab. I.	Turns in a minute.	Expenditure of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
1st	2	86	264,7	1266	$\left\{ \begin{array}{l} \text{Max. 1st, } 262 : 264,7 :: 385 : 389 \\ \text{Max. 3d, } \left\{ \begin{array}{l} 86^2 : 48^2 \\ 7396 : 2304 \end{array} \right\} :: 1266 : 394 \end{array} \right.$	5 —	78 : 79
	24	48	262	385			
2d	1	88	275	1411	$\left\{ \begin{array}{l} \text{Max. 1st, } 114 : 275 :: 117 : 282 \\ \text{Max. 3d, } \left\{ \begin{array}{l} 88^2 : 42^2 \\ 7744 : 1764 \end{array} \right\} :: 1411 : 321 \end{array} \right.$	39 —	7 : 8
	10	42	114	117			
3d	11	84	342	1505	$\left\{ \begin{array}{l} \text{Max. 1st, } 167,5 : 342 :: 212 : 433 \\ \text{Max. 3d, } \left\{ \begin{array}{l} 84^2 : 46^2 \\ 7056 : 2116 \end{array} \right\} :: 1505 : 451 \end{array} \right.$	18 —	24 : 25
	17	46	167,5	212			
4 th	18	72	357	1210	$\left\{ \begin{array}{l} \text{Max. 1st, } 228 : 357 :: 317 : 496 \\ \text{Max. 3d, } \left\{ \begin{array}{l} 72^2 : 48^2 \\ 5184 : 2304 \end{array} \right\} :: 1210 : 538 \end{array} \right.$	42 —	12 : 13
	21	48	228	317			

Maxim 4th. *The aperture being the same, the effect will be nearly as the cube of the velocity of the water.*

This also will appear by comparing the contents of columns 3, 8, and 10; as for

Example 1st, of N°. 1. and N°. 10, viz.

N°.	Turns.	Expence.	Effect.
1 ———	88 ———	275 ———	1411
10 ———	42 ———	114 ———	117

Lemma. It must here be observed, that if water passes out of an aperture, in the same section, but with different velocities; the expence will be proportional to the velocity; and therefore conversely, if the expence is not proportional to the velocity, the section of the water is not the same.

Now comparing the water discharged with the turns of N°. 1. and 10, we shall have $88 : 42 :: 275 : 131,2$; but the water discharged by N°. 10. is only 114 lb. therefore, tho' the fluice was drawn to the same height in N°. 10. as in N°. 1. yet the section of the water passing out, was less in N°. 10. than N°. 1. in the proportion of 114 to 131,2; consequently had the effective aperture or section of the water been the same in N°. 10. as in N°. 1. so that 131,2 lb. of water had been discharged instead of 114, the effect would have been increased in the same proportion; that is,

$$\begin{array}{l}
 \text{by the Lemma, } 88 : 42 :: 275 : 131,2 \\
 \text{by maxim 1st, } 114 : 131,2 :: 117 : 134,5 \\
 \text{and by max. 4th, } \left\{ \begin{array}{l} 83^3 : 42^3 \\ 681472 : 74088 \end{array} \right\} :: 1411 : 153,5
 \end{array}$$

Difference — 19

The

The effect therefore of N°. 10. compared with N°. 1. is less than it ought to be by the present maxim in the ratio of 7 : 8.

The foregoing, and three other similar examples, are contained in the following Table.

Examples.	N°. Tab. I.	Turns in a minute.	Expense of water.	Effect.	Comparison.	Variation.	Proportional Variation.
1 st	{ 1 10 }	88 42	275 114	1411 117	{ Lemma. 88 : 42 :: 275 : 131,2 Max. 1. 114 : 131,2 :: 117 : 134,5 Max. 4. 88 ³ : 42 ³ :: 1411 : 153,5 }	19—	7 : 8
2 ^d	{ 11 17 }	84 46	342 167,5	1505 212	{ Lemma. 84 : 46 :: 342 : 187,3 Max. 1. 167,5 : 187,3 :: 212 : 237 Max. 4. 84 ³ : 46 ³ :: 1505 : 247 }	10—	23 : 24
3 ^d	{ 18 21 }	72 48	357 228	1210 317	{ Lemma. 72 : 48 :: 357 : 238 Max. 1. 228 : 238 :: 317 : 331 Max. 4. 72 ³ : 48 ³ :: 1210 : 355 }	24—	14 : 15
4 ^t	{ 22 24 }	68 48	359 262	1006 385	{ Lemma. 68 : 48 :: 359 : 253,4 Max. 1. 262 : 253,4 :: 385 : 372 Max. 4. 68 ³ : 48 ³ :: 1006 : 354 }	18+	20 : 19

OBSERVATIONS.

Observ. 1st. On comparing column 2d and 4th, Tab. I. it is evident, that the *virtual head* bears no certain proportion to the *head of water*; but that when the aperture is greater, or the velocity of the water issuing therefrom less, they approach nearer to a coincidence: and consequently in the large openings of mills and sluices, where great quantities of water are discharged from moderate heads, the head of water, and virtual head determined from the velocity, will nearly agree, as experience confirms.

Observ. 2d. Upon comparing the several proportions between the *power* and *effect* in column 11th, the most general is that of 10 to 3; the extremes 10 to 3,2 and 10 to 2,8; but as it is observable, that where the quantity of water, or the velocity thereof; that is, where the power is greatest, the 2d term of the ratio is greatest also: we may therefore well allow the proportion subsisting in large works, as 3 to 1.

Observ. 3d. The proportions of *velocities* between the *water* and *wheel* in column 12, are contained in the limits of 3 to 1 and 2 to 1; but as the greater velocities approach the limit of 3 to 1, and the greater quantity of water approach to that of 2 to 1, the best general proportion will be that of 5 to 2.

Observ. 4th. On comparing the numbers in column 13, it appears, that there is no certain ratio between the *load* that the wheel will carry at its *maximum*, and what will totally stop it; but that they are contained within the limits of 20 to 19, and
 3 of,

of 20 to 15; but as the effect approaches nearest to the ratio of 20 to 15, or of 4 to 3, when the power is greatest, whether by increase of velocity, or quantity of water, this seems to be the most applicable to large works: but as the load that a wheel ought to have, in order to work to the best advantage, can be assigned, by knowing the effect it ought to produce, and the velocity it ought to have in producing it; the exact knowledge of the greatest load it will bear, is of the less consequence in practice.

It is to be noted, that in all the examples under the three last of the four preceding maxims, the effect of the lesser power falls short of its due proportion to the greater, when compared by its maxim; except the last example of maxim 4th: and hence, if the experiments are taken strictly, we must infer, that the effects increase and diminish in an higher ratio than those maxims suppose: but as the deviation is not very considerable, the greatest being about 1-8th of the quantity in question; and as it is not easy to make experiments of so compounded a nature with absolute precision; we may rather suppose, that the lesser power is attended with some friction, or works under some disadvantage, which has not been duly accounted for, and therefore we may conclude, that these maxims will hold very nearly, when applied to works in large.

After the experiments above mentioned were tried, the wheel, which had originally 24 floats, was reduced to twelve; which caused a diminution in the effect, on account of a greater quantity of water escaping between the floats and the floor; but a cir-

cular sweep being adapted thereto, of such a length, that one float entered the curve before the preceding one quitted it, the effect came so near to the former, as not to give hopes of advancing it by increasing the number of floats beyond 24 in this particular wheel.

P A R T II.

Concerning OVERSHOT WHEELS.

Read May 24, 1759. **I**N the former part of this essay, we have considered the impulse of a confined stream, acting on *Undershot Wheels*. We now proceed to examine the power and application of water, when acting by its *gravity* on *Overshot Wheels*.

In reasoning without experiment, one might be led to imagine, that however different the mode of application is; yet that whenever the same quantity of water descends thro' the same perpendicular space, that the natural effective power would be equal; supposing the machinery free from friction, equally calculated to receive the full effect of the power, and to make the most of it: for if we suppose the height of a column of water to be 30 inches, and resting upon a base or aperture of one inch square; every cubic inch of water that departs therefrom will acquire the same velocity or *momentum*, from the uniform pressure of 30 cubic inches above it, that one cubic inch let fall from the top will acquire in falling down to the level of the aperture; *viz.* such a velocity as in a contrary direction would carry it to
the

the level from whence it fell ; * one would therefore suppose, that a cubic inch of water, let fall thro' a space of 30 inches, and there impinging upon another body, would be capable of producing an equal effect by collision, as if the same cubic inch had descended thro' the same space with a slower motion, and produced its effects gradually: for in both cases gravity acts upon an equal quantity of matter, thro' an equal space †; and consequently, that whatever was the ratio between the power and effect in undershot wheels, the same would obtain in overshot, and indeed in all others: yet, however conclusive this reasoning may seem, it will appear, in the course of the following deductions, that the effect of the gravity of descending bodies is very different from the effect of the stroke of such as are *non-elastic*, tho' generated by an equal mechanical power.

The alterations in the machinery already described, to accommodate the same for experiments on overshot wheels, were principally as follows.

PLATE V. *Fig. 2.* The sluice *Ib* being shut down, the rod *HI* was unscrewed and taken off.

The undershot water-wheel was taken off the axis, and instead thereof an overshot wheel of the same

* This is a consequence of the rising of jets to the height of their reservoirs nearly.

† Gravity, it is true, acts a longer space of time upon the body that descends slow than upon that which falls quick; but this cannot occasion the difference in the effect: for an elastic body falling thro' the same space in the same time, will, by collision upon another elastic body, rebound nearly to the height from which it fell; or, by communicating its motion, cause an equal one to ascend to the same height.

diameter

diameter was put into its place. *Note*, This wheel was two inches in the shroud or depth of the bucket ; the number of the buckets was 36.

The standards S and T, *Fig. 1.* were raised half an inch, so that the bottom of the wheel might be clear of stagnant water.

A trunk, for bringing the water upon the wheel, was fixed according to the dotted lines *f g*, *Fig. 2.* The aperture was adjusted by a shuttle *b i*, which also closed up the outer end of the trunk, when the water was to be stopped.

Fig. 3. The ratchet *o o*, not being of one piece of metal with the ferrule *e e*, *i i* (tho' so described before, to prevent unnecessary distinctions), was with its catch turned the contrary side ; consequently the moveable barrel would do its office equally, notwithstanding the water-wheel, when at work, moved the contrary way.

Specimen of a Sett of Experiments.

Head 6 inches.

14 $\frac{1}{2}$ strokes of the pump in a minute, 12 ditto =
80 lb. *

Weight of the scale (being wet) 10 $\frac{1}{2}$ oz.

Counterweight for 20 turns, besides the scale, 3 oz.

No.	Weight in the Scale.	Turns.	Product.	Observations.
1	0 lb.	60		Threw most part of the water out of the wheel.
2	1	56		
3	2	52		
4	3	49	147	Received the wa- ter more quietly.
5	4	47	188	
6	5	45	225	
7	6	42 $\frac{1}{2}$	255	
8	7	41	287	
9	8	38 $\frac{1}{2}$	308	
10	9	36 $\frac{1}{2}$	328 $\frac{1}{2}$	
11	10	35 $\frac{1}{2}$	355	
12	11	32 $\frac{3}{4}$	360 $\frac{1}{2}$	
13	12	31 $\frac{1}{4}$	375	
14	13	28 $\frac{1}{2}$	370 $\frac{1}{4}$	
15	14	27 $\frac{1}{2}$	385	
16	15	26	390	
17	16	24 $\frac{1}{2}$	392	
18	17	22 $\frac{3}{4}$	386 $\frac{3}{4}$	
19	18	21 $\frac{3}{4}$	391 $\frac{1}{2}$	
20	19	20 $\frac{3}{4}$	394 $\frac{1}{4}$	Maximum.
21	20	19 $\frac{3}{4}$	395	
22	21	18 $\frac{1}{4}$	388 $\frac{1}{4}$	
23	22	18	396	Work'd irregular.
24	23	Overfet by its load.		

* The small difference, in the value of 12 strokes of the pump, from the former experiments, was owing to a small difference in the length of the stroke, occasioned by the warping of the wood.

Reduction.

Reduction of the preceding Specimen.

In these experiments the head being 6 inches, and the height of the wheel 24 inches, the whole descent will be 30 inches: the expence of water was $14\frac{1}{2}$ strokes of the pump in a minute, whereof 12 contained 80 lb.; therefore the water expended in a minute was $96\frac{2}{3}$ lb. which, multiplied by 30 inches, gives the *power* = 2900.

If we take the 20th experiment for the *maximum*, we shall have $20\frac{3}{4}$ turns in a minute, each of which raised the weight $4\frac{1}{2}$ inches, that is, 93,37 inches in a minute. The weight *in* the scale was 19 lb, the weight *of* the scale $10\frac{1}{2}$ oz.; the counter-weight 3 oz. in the scale, which, with the weight of the scale $10\frac{1}{2}$ oz. makes in the whole $20\frac{1}{2}$ lb. which is the whole resistance or load: this, multiplied by 93,37 inches, makes 1914 for the effect.

The *ratio* therefore of the *power* and *effect* will be as 2900 : 1914, or as 10 : 6,6, or as 3 : 2 nearly.

But if we compute the power from the height of the wheel only, we shall have $96\frac{2}{3}$ lb. multiplied by 24 inches = 2320 for the *power*, and this will be to the *effect* as 2320 : 1914, or as 10 : 82, or as 5 : 4 nearly.

The reduction of this specimen is set down in N^o. 9. of the following Table; and the rest were deducted from a similar sett of experiments, reduced in the same manner.

TABLE II. *containing the Result of Sixteen Sets of Experiments on Overshot Wheels.*

N ^o .	Whole descent.	Water expended in a minute.	Turns at the maximum in a min.	Weight raised at the maximum.	Power of the whole descent.	Power of the wheel.	Eff ^{ct} .	Ratio of the whole power and effect.	Ratio of power of the wheel and effect.	Mean ratio.
	<i>Inch</i>	<i>lb.</i>		<i>lb.</i>						
1	27	30	19	6 $\frac{1}{2}$	810	720	556	10:6,9	10:7,7	
2	27	56 $\frac{2}{3}$	16 $\frac{1}{4}$	14 $\frac{1}{2}$	1530	1360	1060	10:6,9	10:7,8	
3	27	56 $\frac{2}{3}$	20	12 $\frac{1}{2}$	1530	1360	1167	10:7,6	10:8,4	
4	27	63 $\frac{1}{3}$	20 $\frac{1}{2}$	13 $\frac{1}{2}$	1710	1524	1245	10:7,5	10:8,2	
5	27	76 $\frac{2}{3}$	21 $\frac{1}{2}$	15 $\frac{1}{2}$	2070	1840	1500	10:7,3	10:8,2	
6	28 $\frac{1}{2}$	73 $\frac{1}{3}$	18 $\frac{3}{4}$	17 $\frac{1}{2}$	2090	1764	1476	10:7,	10:8,4	
7	28 $\frac{1}{2}$	96 $\frac{2}{3}$	20 $\frac{1}{4}$	20 $\frac{1}{2}$	2755	2320	1868	10:6,8	10:8,1	
8	30	90	20	19 $\frac{1}{2}$	2700	2160	1755	10:6,5	10:8,1	
9	30	96 $\frac{2}{3}$	20 $\frac{1}{4}$	20 $\frac{1}{2}$	2900	2320	1917	10:6,6	10:8,2	
10	30	113	21	23 $\frac{1}{2}$	3400	2720	2221	10:6,5	10:8,2	
11	33	56 $\frac{2}{3}$	20 $\frac{1}{4}$	13 $\frac{1}{2}$	1870	1360	1230	10:6,6	10:9,	
12	33	106 $\frac{2}{3}$	22 $\frac{1}{4}$	21 $\frac{1}{2}$	3520	2560	2153	10:6,1	10:8,4	
13	33	146 $\frac{2}{3}$	23	27 $\frac{1}{2}$	4840	3520	2846	10:5,9	10:8,1	
14	35	65	19 $\frac{3}{4}$	16 $\frac{1}{2}$	2275	1560	1466	10:6,5	10:9,4	
15	35	120	21 $\frac{1}{2}$	25 $\frac{1}{2}$	4200	2880	2467	10:5,9	10:8,6	
16	35	163 $\frac{1}{2}$	25	26 $\frac{1}{2}$	5728	3924	2981	10:5,2	10:7,6	
I	2.	3	4	5.	6.	7.	8.	9.	10	11

Observations and Deductions from the foregoing Experiments.

I. Concerning the Ratio between the Power and Effect of Overshot Wheels.

The effective power of the water must be reckoned upon the whole descent; because it must be

raised that height, in order to be in a condition of producing the same effect a second time.

The ratio's between the *powers* so estimated, and the *effects* at the *maximum* deduced from the several sets of experiments, are exhibited at one view in column 9. of Table II.; and from hence it appears, that those ratio's differ from that of 10 to 7,6 to that of 10 : 5,2, that is, nearly from 4 : 3 to 4 : 2. In those experiments where the heads of water and quantities expended are least, the proportion is nearly as 4 : 3 ; but where the heads and quantities are greatest, it approaches nearer to that of 4 : 2 ; and by a medium of the whole, the ratio is that of 3 : 2 nearly. We have seen before, in our observations upon the effects of undershot wheels, that the general ratio of the power to the effect, when greatest, was 3 : 1 ; *the effect therefore of overshot wheels, under the same circumstances of quantity and fall, is at a medium double to that of the undershot* : and, as a consequence thereof, *that nonelastic bodies, when acting by their impulse or collision, communicate only a part of their original power* ; the other part being spent in changing their figure in consequence of the stroke.

The powers of water computed from the height of the wheel only, compared with the effects, as in column 10. appear to observe a more constant ratio : for if we take the medium of each class, which is set down in column 11, we shall find the extremes to differ no more than from the ratio of 10 : 8,1 to that of 10 : 8,5 ; and as the second term of the ratio gradually increases from 8,1 to 8,5, by an increase of head from 3 inches to 11, the excess of 8,5 above

8,1 is to be imputed to the superior impulse of the water at the head of 11 inches above that of 3 inches: so that if we reduce 8,1 to 8, on account of the impulse of the 3 inch head, *we shall have the ratio of the power, computed upon the height of the wheel only, to the effect at a maximum as 10:8, or as 5:4 nearly*: and from the equality of the ratio between power and effect, subsisting where the constructions are similar, we must infer, *that the effects, as well as the powers, are as the quantities of water and perpendicular heights multiplied together respectively.*

II. Concerning the most proper Height of the Wheel in proportion to the whole Descent.

We have already seen, from the preceding observation, that the effect of the same quantity of water, descending thro' the same perpendicular space, is double, when acting by its gravity upon an overshot wheel, to what the same produces when acting by its impulse upon an undershot. It also appears, that by increasing the head from 3 inches to 11, that is, the whole descent, from 27 inches to 35, or in the ratio of 7 to 9 nearly, the effect is advanced no more than in the ratio of 8,1 to 8,4, that is, as 7:7,26; and consequently the increase of effect as not 1-7th of the increase of perpendicular height. Hence it follows, *that the higher the wheel is in proportion to the whole descent, the greater will be the effect*; because it depends less upon the impulse of the head, and more upon the gravity of the water in the buckets: and if we consider how obliquely the water issuing from the head must strike the buckets, we shall not be at a loss to account for the little ad-

vantage that arises from the impulse thereof; and shall immediately see of how little consequence this impulse is to the effect of an overshot wheel. However, as every thing has its limits, so has this: for thus much is desirable, *that the water should have somewhat greater velocity, than the circumference of the wheel, in coming thereon*; otherwise the wheel will not only be retarded, by the buckets striking the water, but thereby dashing a part of it over, so much of the power is lost.

The velocity that the circumference of the wheel ought to have, being known by the following deductions, the head requisite to give the water its proper velocity is easily computed from the common rules of hydrostatics; and will be found much less than what is generally practised.

III. *Concerning the Velocity of the Circumference of the Wheel, in order to produce the greatest Effect.*

If a body is let fall freely from the surface of the head to the bottom of the descent, it will take a certain time in falling; and in this case the whole action of gravity is spent in giving the body a certain velocity: but if this body in falling is made to act upon some other body, so as to produce a mechanical effect, the falling body will be retarded; because a part of the action of gravity is then spent in producing the effect, and the remainder only giving motion to the falling body: and therefore *the slower a body descends, the greater will be the portion of the action of gravity applicable to the producing a mechanical effect*; and in consequence the greater that effect may be.

If a stream of water falls into the bucket of an overshot wheel, it is there retained till the wheel by moving round discharges it: of consequence the slower the wheel moves, the more water each bucket will receive: so that what is lost in speed, is gained by the pressure of a greater quantity of water acting in the buckets at once: and, if considered only in this light, the mechanical power of an overshot wheel to produce effects will be equal, whether it moves quick or slow: but if we attend to what has been just now observed of the falling body, it will appear that so much of the action of gravity, as is employed in giving the wheel and water therein a greater velocity, must be subtracted from its pressure upon the buckets; so that, tho' the product made by multiplying the number of cubic inches of water acting in the wheel at once by its velocity will be the same in all cases; yet, as each cubic inch, when the velocity is *greater* does not press so much upon the bucket as when it is *less*, the power of the water to produce effects will be greater in the less velocity than in the greater: and hence we are led to this general rule, *that, ceteris paribus, the less the velocity of the wheel, the greater will be the effect thereof.* A confirmation of this doctrine, together with the limits it is subject to in practice, may be deduced from the foregoing specimen of a sett of experiments.

From these experiments it appears, that when the wheel made about 20 turns in a minute, the effect was, near upon, the greatest. When it made 30 turns, the effect was diminished about $\frac{1}{20}$ part; but that when it made 40, it was diminished about $\frac{1}{4}$; when it made less than 18 $\frac{1}{4}$, its motion was irregular; and when

when it was loaded so as not to admit its making 18 turns, the wheel was overpowered by its load.

It is an advantage in practice, that the velocity of the wheel should not be diminished further than what will procure some solid advantage in point of power; because, *cæteris paribus*, as the motion is slower, the buckets must be made larger; and the wheel being more loaded with water, the stress upon every part of the work will be increased in proportion: *The best velocity for practice therefore will be such, as when the wheel here used made about 30 turns in a minute; that is, when the velocity of the circumference is a little more than 3 feet in a second.*

Experience confirms, that this velocity of 3 feet in a second is applicable to the highest overshot wheels, as well as the lowest; and all other parts of the work being properly adapted thereto, will produce very nearly the greatest effect possible: however this also is certain from experience, that *high wheels may deviate further from this rule, before they will lose their power, by a given aliquot part of the whole, than low ones can be admitted to do*; for a wheel of 24 feet high may move at the rate of six feet per second without losing any considerable part of its power*; and, on the other hand, I have seen a wheel of 33 feet high, that has moved very steadily and well with a velocity but little exceeding 2 feet.

* The 24 feet wheel going at 6 feet in a second seems owing to the small proportion that the head (requisite to give the water the proper velocity of the wheel) bears to the whole height.

IV. *Concerning the Load for an Overshot Wheel, in order that it may produce a Maximum.*

The maximum load for an overshot wheel, is that which reduces the circumferences of the wheel to its proper velocity; and this will be known, by dividing the effect it ought to produce in a given time by the space intended to be described by the circumference of the wheel in the same time: the quotient will be the resistance overcome at the circumference of the wheel; and is equal to the load required, the friction and resistance of the machinery included.

V. *Concerning the greatest possible Velocity of an Overshot Wheel.*

The greatest velocity that the circumference of an overshot wheel is capable of, depends jointly upon the diameter or *height* of the wheel, and the velocity of falling bodies; for it is plain that the velocity of the circumference can never be greater, than to describe a semi-circumference, while a body let fall from the top of the wheel will descend thro' its diameter; nor indeed quite so great, as a body descending thro' the same perpendicular space cannot perform the same in so small a time when passing thro' a semi-circle, as would be done in a perpendicular line. Thus, if a wheel is 16 feet 1 inch high, a body will fall thro' the diameter in one second: this wheel therefore can never arrive at a velocity equal to the making one turn in two seconds; but, in reality, an overshot wheel can never come near this velocity; for when it acquires a certain speed,
the

the greatest part of the water is prevented from entering the buckets; and the rest, at a certain point of its descent, is thrown out again by the centrifugal force. This appears to have been the case in the three first experiments of the foregoing specimen; but as the velocity, when this begins to happen, depends upon the form of the buckets, as well as other circumstances, *the utmost velocity of overshot wheels is not to be determined generally*: and, indeed, it is the less necessary in practice, as it is in this circumstance incapable of producing any *mechanical effect*, for reasons already given.

VI. *Concerning the greatest Load that an Overshot Wheel can overcome.*

The greatest load an overshot wheel will overcome, considered abstractedly, is unlimited or infinite: for as the buckets may be of any given capacity, the more the wheel is loaded, the slower it turns; but the slower it turns, the more will the buckets be filled with water; and consequently tho' the diameter of the wheel, and quantity of water expended, are both limited, yet no resistance can be assigned, which it is not able to overcome: but in practice we always meet with something that prevents our getting into infinitesimals; for when we really go to work to build a wheel, the buckets must necessarily be of some given capacity; and consequently *such a resistance will stop the wheel, as is equal to the effort of all the buckets in one semi-circumference filled with water.*

The structure of the buckets being given, the quantity of this effort may be assigned; but is not of much consequence to the practice, as in this case
also

also the wheel loses its power; for tho' here is the exertion of gravity upon a given quantity of water, yet being prevented by a counterbalance from moving, is capable of producing no *mechanical effect*, according to our definition. But, in reality, an overshot wheel generally ceases to be useful before it is loaded to that pitch; for *when it meets with such a resistance as to diminish its velocity to a certain degree, its motion becomes irregular; yet this never happens till the velocity of the circumference is less than 2 feet per second, where the resistance is equable*, as appears not only from the preceding specimen, but from experiments on larger wheels.

SCHOLIUM.

- Having now examined the different effects of the power of water, when acting by its *impulse*, and by its *weight*, under the titles of *undershot* and *overshot* wheels; we might naturally proceed to examine the effects when the impulse and weight are combined, as in the several kinds of *breast-wheels*, &c. but, what has been already delivered being carefully attended to, the application of the same principles in these mixt cases will be easy, and reduce what I have to say on this head into a narrow compass: for all kinds of wheels where the water cannot descend thro' a given space, unless the wheel moves therewith, are to be considered of the nature of an overshot wheel, according to the perpendicular height that the water descends from; and all those that receive the impulse or shock of the water, whether in an horizontal, perpendicular, or oblique direction, are to be considered as undershots. And therefore a wheel, which the

water strikes at a certain point below the surface of the head, and after that descends in the arch of a circle, pressing by its gravity upon the wheel; *the effect of such a wheel will be equal to the effect of an undershot, whose head is equal to the difference of level between the surface of the water in the reservoir and the point where it strikes the wheel, added to that of an overshot, whose height is equal to the difference of level, between the point where it strikes the wheel and the level of the tail-water.* It is here supposed, that the wheel receives the shock of the water at right angles to its radii; and that the velocity of its circumference is properly adapted to receive the utmost advantage of both these powers; otherwise a reduction must be made on that account.

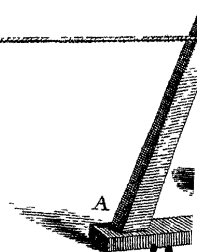
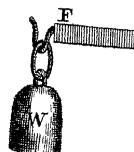
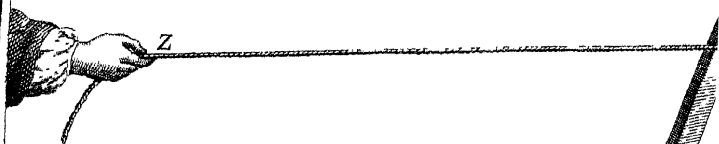
Many obvious and considerable improvements upon the common practice naturally offer themselves, from a due consideration of the principles here established, as well as many popular errors show themselves in view: but as my present purpose extends no farther than the laying down such general rules as will be found to answer in practice, I leave the particular application to the intelligent artist, and to the curious in these matters.

P A R T III.

On the Construction and Effects of WINDMILL-SAILS.

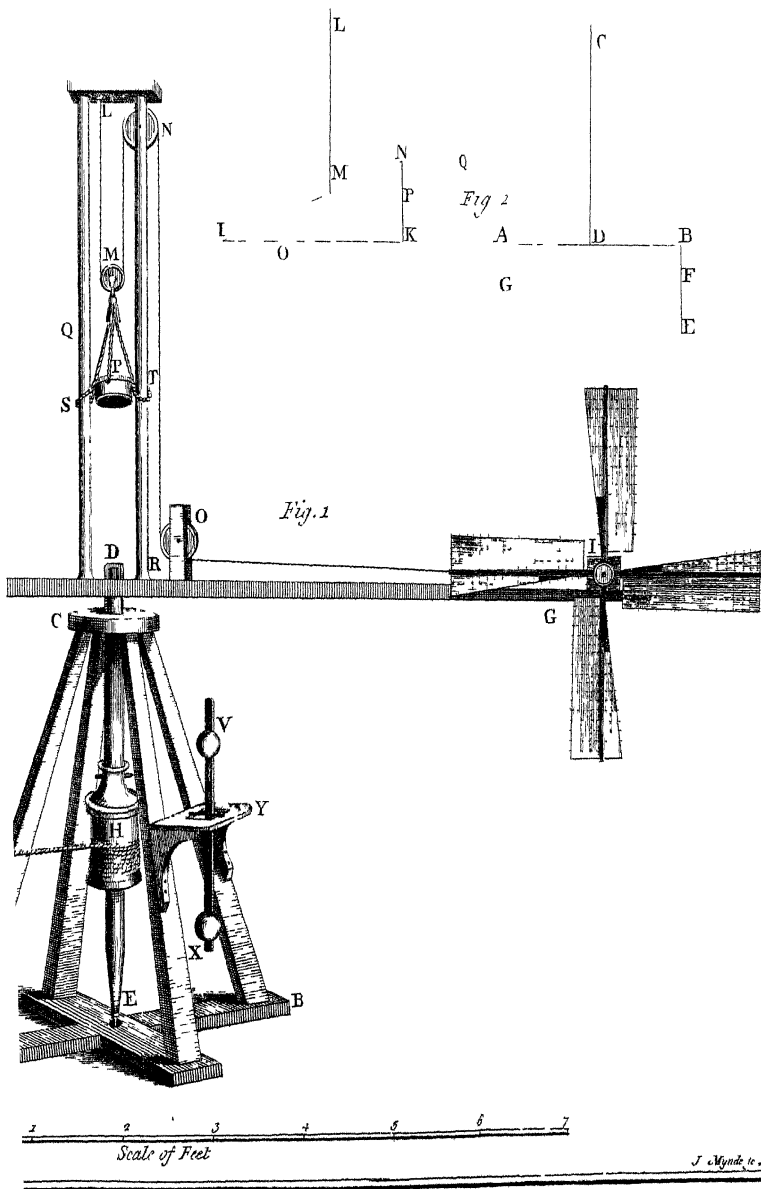
Read 31 May & 14 June, 1759. **I**N trying experiments on windmill-fails, the wind itself is too uncertain to answer the purpose: we must therefore have recourse to an artificial wind.

This



J. Smardon delin.





This may be done two ways; either by causing the air to move against the machine, or the machine to move against the air. To cause the air to move against the machine, in a sufficient column, with steadiness and the requisite velocity, is not easily put in practice: To carry the machine forward in a right line against the air, would require a larger room than I could conveniently meet with. What I found most practicable, therefore, was, to carry the axis, whereon the sails were to be fixed, progressively round in the circumference of a large circle. Upon this idea * a machine was constructed, as follows.

PLATE VI. *Fig. 1.*

ABC is a pyramidal frame for supporting the moving parts.

DE is an upright axis, whereon is framed

FG, an arm for carrying the sails at a proper distance from the center of the upright axis.

* Some years ago Mr. Rouse, an ingenious gentleman of Harborough in Leicestershire, set about trying experiments on the velocity of the wind, and force thereof upon plain surfaces and windmill-sails: and much about the same time Mr. Ellicott contrived a machine for the use of the late celebrated Mr. B. Robins, for trying the resistance of plain surfaces moving thro' the air. The machines of both these gentlemen were much alike, tho' at that time totally unacquainted with each other's inquiries. But it often happens, that when two persons think justly upon the same subject, their experiments are alike. This machine was also built upon the same idea as the foregoing; but differed in having the hand for the first mover, with a pendulum for its regulator, instead of a weight, as in the former; which was certainly best for the purposes of measuring the impulse of the wind, or resistance of plains: but the latter is more applicable to experiments on windmill-sails; because every change of position of the same sails will occasion their meeting the air with a different velocity, tho' urged by the same weight.

H is a barrel upon the upright axis, whereon is wound a cord ; which, being drawn by the hand, gives a circular motion to the axis, and to the arm F G ; and thereby carries the axis of the fails in the circumference of a circle, whose radius is D I, causing thereby the fails to strike the air, and turn round upon their own axis.

At L is fixed the end of a small line, which passing through the pullies M N O, terminates upon a small cylinder or barrel upon the axis of the fails, and, by winding thereon, raises

P the scale, wherein the weights are placed for trying the power of the fails. This scale, moving up and down in the direction of the upright axis, receives no disturbance from the circular motion.

Q R two parallel pillars standing upon the arm F G, for the purpose of supporting and keeping steady the scale P ; which is kept from swinging by means of

S T two small chains, which hang loosely round the two pillars.

W is a weight, for bringing the center of gravity of the moveable part of the machine into the center of motion of the axis D E.

V X is a pendulum, composed of two balls of lead, which are moveable upon a wooden rod, and thereby can be so adjusted, as to vibrate in any time required. This pendulum hangs upon a cylindrical wire, whereon it vibrates, as on a rolling axis.

Y is a perforated table for supporting the axis of the pendulum.

Note, The pendulum being so adjusted, as to make two vibrations in the time that the arm FG is intended to make one turn; the pendulum being set a vibrating, the experimenter pulls by the cord Z, with sufficient force to make each half revolution of the arm to correspond with each vibration, as equal as possible, during the number of vibrations that the experiment is intended to be continued. A little practice renders it easy to give motion thereto with all the regularity that is necessary.

Specimen of a Sett of Experiments.

Radius of the fails	— — —	21 inches
Length of ditto in the cloth	— —	18
Breadth of ditto	— — —	5,6
* { Angle at the extremity	— — —	10 degrees
* { Ditto at the greatest inclination	—	25
20 turns of the fails raised the weight		11, 3 inches
Velocity of the center of the fails, in the circumference of the great circle, in a second	— — — — —	} 6 ft. 0 in.
Continuance of the experiment	—	
		52 seconds.

N ^o .	Wt. in the scale.	Turns.	Product.
1	— 0 lb. —	108	— 0
2	— 6 —	85	— 510
3	— 6 $\frac{1}{2}$ —	81	— 526 $\frac{1}{2}$
4	— 7 —	78	— 546
5	— 7 $\frac{1}{2}$ —	73	— 547 $\frac{1}{2}$ maxim ^m
6	— 8 —	65	— 520
7	— 9 —	0	— 0

* In all the following experiments the angle of the fails is accounted from the plain of their motion; that is, when they stand at right angles to the axis, their angle is denoted 0°, this notation being agreeable to the language of practitioners, who call the angle so denoted, the weather of the fail; which they denominate greater or less, according to the quantity of this angle.

N. B.

N. B. The weight of the scale and pulley was 3 oz.; and that 1 oz. suspended upon one of the radii, at $12\frac{1}{2}$ inches from the center of the axis, just overcame the friction scale and load of $7\frac{1}{2}$ lb.; and placed at $14\frac{1}{2}$ inches, overcame the same resistances with 9 lb. in the scale.

Reduction of the preceding Specimen.

N^o. 5. being taken for the maximum, the weight in the scale was 7 lb. 8 oz. which, with the weight of the scale and pulley 3 oz. makes 7 lb. 11 oz. equal to 123 oz.; this added to the friction of the machinery, the sum is the whole resistance *. The friction of the machinery is thus deduced: Since 20 turns of the sails raised the weight 11,3 inches, with a double line, the radius of the cylinder will be .18 of an inch; but had the weight been raised by a single line, the radius of the cylinder being half the former, viz. .09, the resistance would have been the same: we shall therefore have this analogy; as half the radius of the cylinder, is to the length of the arm where the small weight was applied; so is the weight applied to the arm, to a fourth weight, which is equivalent to the sum of the whole resistance together; that is, .09 : 12,5 :: 1 oz. : 139 oz.: this exceeds 123 oz. the weight in the scale, by 16 oz. or 1 lb. which is equivalent to the friction; and which, added to the above weight of 7 lb. 11 oz. makes 8 lb. 11 oz. = 8,69 lb. for the sum of the whole re-

* The resistance of the air is not taken into the account of resistance, because it is inseparable from the application of the power.

distance ; and this, multiplied by 73 turns, makes a product of 634, which may be called the representative of the *effect* produced.

In like manner, if the weight 9 lb. which caused the sails to rest after being in motion, be augmented by the weight of the scale and its relative friction, it will become 10,37 lb. The result of this specimen is set down in N^o. 12. of Table III. and the result of every other set of experiments therein contained were made and reduced in the same manner.

TABLE III. Containing Nineteen Sets of Experiments on Windmill-Sails of various Structures, Positions, and Quantities of Surfaces.

The kind of sails made use of.	N ^o .	Angle at the extremities.	Greatest angle.	Turns of the sails unloaded.	Turns of ditto at the maxim ^m .	Load at the maximum.	Greatest load.	Product.	Quantity of surface.	Ratio of greatest velocity to the velocity at a maximum.	Ratio of greatest load to the load at maximum.	Ratio of surface to the product.
Plain sails at an angle of 55°.	1	35	35	66	42	7,56	12,59	318	404	10:7	10:6	10:7,9
Plain sails weather'd according to the common practice.	2	12	12		70	6,3	7,56	441	404		10:8,3	10:10,1
	3	15	15	105	60	6,72	8,12	464	404	10:6,6	10:8,3	10:10,15
	4	18	18	96	66	7,0	9,81	462	404	10:7,	10:7,1	10:10,15
Weathered according to Maclaurin's theorem.	5	9	26½		66	7,0		462	404			10:11,4
	6	12	29½		70½	7,35		518	404			10:12,8
	7	15	32½		63½	8,3		527	404			10:13,
Sails weathered in the Dutch manner, tried in various positions.	8	0	15	120	93	4,75	5,31	442	404	10:7,7	10:8,9	10:11,
	9	3	18	120	79	7,0	8,12	553	404	10:6,6	10:8,6	10:13,7
	10	5	20		78	7,5	8,12	585	404		10:9,2	10:14,5
	11	7½	22½	113	77	8,3	9,81	639	404	10:6,8	10:8,5	10:15,8
	12	10	25	108	73	8,69	10,37	634	404	10:6,8	10:8,4	10:15,7
	13	12	27	100	66	8,41	10,94	580	404	10:6,6	10:7,7	10:14,4
Sails weathered in the Dutch manner, but enlarged towards the extremities.	14	7½	22½	123	75	10,65	12,59	799	505	10:6,1	10:8,5	10:15,8
	15	10	25	117	74	11,08	13,69	820	505	10:6,3	10:8,1	10:16,2
	16	12	27	114	66	12,09	14,23	799	505	10:5,8	10:8,4	10:15,8
	17	15	30	96	63	12,09	14,78	762	505	10:6,6	10:8,2	10:15,1
3 sails being sectors of ellipses in their best positions.	18	12	22	105	64½	16,42	27,87	1059	854	10:6,1	10:5,9	10:12,4
	19	12	22	99	64½	18,06		1165	1146	10:5,9		10:10,1
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.

Observations and Deductions from the preceding Experiments.

I. Concerning the best Form and Position of Wind-mill-Sails.

In Table III. N^o. 1. is contained the result of a sett of experiments upon sails set at the angle which the celebrated Monsr. Parint, and succeeding geometricians for many years, held to be the best; *viz.* those whose planes make an angle 55° nearly with the axis; the complement whereof, or angle that the plane of the sail makes with the plane of their motion, will therefore be 35° , as set down in col. 2. and 3. Now if we multiply their number of turns by the weight they lifted, when working to the greatest advantage, as set down in columns 5. and 6. and compare this product (col. 8.) with the other products contained in the same column, instead of being the greatest, it turns out the least of all the rest. But if we set the angle of the same planes at somewhat less than half the former, or at any angle from 15° to 18° , as in N^o. 3. and 4. that is, from 72° to 75° with the axis, the product will be increased in the ratio of 31 : 45; and this is the angle most commonly made use of by practitioners, when the surfaces of the sails are planes.

If nothing more was intended than to determine the most efficacious angle to make a mill acquire motion from a state of rest, or to prevent it from passing into rest from a state of motion, we shall find the position of N^o. 1. the best; for if we consult col. 7. which contains the least weights, that would make the sails pass from motion to rest, we shall find that of N^o. 1.

(relative to the quantity of cloth) the greatest of all. But if the sails are intended, with given dimensions, to produce the greatest effect possible in a given time, we must intirely reject those of N^o. 1. and, *if we are confined to the use of planes, conform ourselves to some angle between N^o. 3. and 4. that is, not less than 72°, or greater than 75°, with the axis.*

The late celebrated Mr. Maclaurin has judiciously distinguished between the action of the wind upon a sail at rest, and a sail in motion; and, in consequence, as the motion is more rapid near the extremities than towards the center, that the angle of the different parts of the sail, as they recede from the center, should be varied. For this purpose he has furnished us with the following theorem*. “ Suppose the velocity
“ of the wind to be represented by a , and the velocity of any given part of the sail to be denoted by
“ c ; then the effort of the wind upon that part of
“ the sail will be greatest when the tangent of the
“ angle, in which the wind strikes it, is to radius as
“ $\sqrt{2 + \frac{9c^2}{4a^2} + \frac{3c}{2a}}$ to 1.” This theorem then assigns the law, by which the angle is to be varied according to the velocity of each part of the sail to the wind: but as it is left undetermined what velocity any one given part of the sail ought to have in respect to the wind, the angle that any one part of the sail ought to have, is left undetermined also; so that we are still at a loss for the proper *data* to apply the theorem. However, being willing to avail myself thereof, and considering that any angle from 15° to 18° was best suited to a plane, and of consequence the best

* Maclaurin’s account of Sir Isaac Newton’s philosophical discoveries, p. 176, art. 29.

mean angle, I made the sail, at the middle distance between the center and the extremity, to stand at an angle of $15^{\circ} 41'$ with the plane of the motion; in which case the velocity of that part of the sail, when loaded to a *maximum*, would be equal to that of the wind, or $c = a$. This being determined, the rest were inclined according to the theorem, as follows :

Parts of the radius from the center.	Angle with the axis.		Angle of weather.		
$\frac{1}{6} -- c = \frac{1}{3} a$	--	$63^{\circ} 26'$	--	$26^{\circ} 34'$	
$\frac{2}{6} -- c = \frac{2}{3} a$	--	$69 \ 54$	--	$20 \ 6$	
$\frac{1}{2} -- c = a$	--	$74 \ 19$	--	$15 \ 41$	middle
$\frac{2}{3} -- c = 1\frac{1}{3} a$	--	$77 \ 20$	--	$12 \ 40$	
$\frac{5}{6} -- c = 1\frac{2}{3} a$	--	$79 \ 27$	--	$10 \ 33$	
$1 -- c = 2a$	--	$81 \ 0$	--	$9 \ 0$	extremity.

- The result hereof was according to N^o. 5. being nearly the same as the plane sails, in their best position : but being turned round in their sockets, so that every part of each sail stood at an angle of 3° , and afterwards of 6° , greater than before, that is, their extremities being moved from 9° to 12° and 15° , the products were advanced to 518 and 527 respectively. Now from the small difference between those two products, we may conclude, that they were nearly in their best position, according to N^o. 7. or some angle between that and N^o. 6 : but from these, as well as the plane sails and others, we may also conclude, that *a variation in the angle of a degree or two makes very little difference in the effect, when the angle is near upon the best.*

It is to be observed, that a sail inclined by the preceding rule will expose a convex surface to the wind : whereas the Dutch, and all our modern

mill-builders, tho' they make the angle to diminish, in receding from the center towards the extremity, yet constantly do it in such manner, as that the surface of the sail may be concave towards the wind. In this manner the sails made use of in N^o. 8, 9, 10, 11, 12, and 13. were constructed; the middle of the sail making an angle with the extreme bar of 12° ; and the greatest angle (which was about $\frac{1}{3}$ of the radius from the centre) of 15° therewith. Those sails being tried in various positions, the best appears to be that of N^o. 11. where the extremities stood at an angle of $7^{\circ}\frac{1}{2}$ with the plane of motion, the product being 639: greater than that of those made by the theorem in the ratio of 9:11, and double to that of N^o. 1.; and this was the greatest product that could be procured without an augmentation of surface.⁴ Hence it appears, that *when the wind falls upon a concave surface, it is an advantage to the power of the whole, tho' every part, taken separately, should not be disposed to the best advantage* *.

Having thus obtained the best position of the sails, or manner of weathering, as it is called by workmen, the next point was to try what advantage could be

* By several trials in large I have found the following angles to answer as well as any. The radius is supposed to be divided into 6 parts and 1-6th, reckoning from the center, is called 1, the extremity being denoted 6.

N ^o .	Angle with the axis.	Angle with the plane of motion.
1	72°	18°
2	71	19
3	72	18 middle.
4	74	16
5	77 $\frac{1}{2}$	12 $\frac{1}{2}$
6	83	7 extremity.

made

made by an addition of surface upon the same radius. For this purpose, the sails made use of had the same weather as those N^o. 8. to 13, with an addition to the leading side of each of a triangular cloth, whose height was equal to the height of the sail, and whose base was equal to half the breadth: of consequence the increase of surface upon the whole was one fourth part, or as 4 : 5. Those sails, by being turned round in their sockets, were tried in four different positions, specified in N^o. 14, 15, 16, and 17; from whence it appears, that the best was when every part of the sail made a greater angle by $2^{\circ} \frac{1}{2}$, with the plane of the motion, than those without the addition, as appears by N^o. 15. the product being 826 : this exceeds 639 more than in the ratio of 4 : 5, or that of the increase of cloth. Hence it appears, that *a broader sail requires a greater angle*; and that *when the sail is broader at the extremity, than near the center, this shape is more advantageous than that of a parallelogram* *.

Many have imagined, that the more sail, the greater the advantage, and have therefore proposed to fill up the whole area: and by making each sail a sector of an ellipsis, according to Monsieur Parint, to intercept the whole cylinder of wind, and thereby to produce the greatest effect possible.

* The figure and proportion of the enlarged sails, which I have found best to answer in large, are represented in the figure, Plate VI. where the extreme bar is $\frac{1}{3}$ d of the radius (or whip, as it is called by the workmen), and is divided by the whip in the proportion of 3 to 5. The triangular or leading sail is covered with board from the point downwards $\frac{1}{3}$ d of its height, the rest with cloth as usual. The angles of weather in the preceding note are best for the enlarged sails also; for in practice it is found, that the sails had better have too little than too much weather.

We have therefore proceeded to inquire, how far the effect could be increased by a further enlargement of the surface, upon the same radius of which N^o. 18 and 19 are specimens. The surfaces indeed were not made planes, and set at an angle of 35° , as Parint proposed; because, from N^o. 1. we learn, that this position has nothing to do, when we intend them to work to the greatest advantage. We therefore gave them such an angle as the preceding experiments indicated for such sort of sails, *viz.* 12° at the extremity, and 22° for the greatest weather. By N^o. 18 we have the product 1059, greater than N^o. 15. in the ratio of 7 : 9; but then the augmentation of cloth is almost 7 : 12. By N^o. 19. we have the product 1165, that is greater than N^o. 15. as 7 : 10; but the augmentation of cloth is nearly as 7 : 16; consequently had the same quantity of cloth as in N^o. 18. been disposed in a figure similar to that of N^o. 15, instead of the product 1059, we should have had the product 1386; and in N^o. 19, instead of the product 1165, we should have had a product of 1860; as will be further made appear in the course of the following deductions. Hence it appears, that beyond a certain degree, the more the area is crowded with sail, the less effect is produced in proportion to the surface: and by pursuing the experiments still further, I found, that tho' in N^o. 19. the surface of all the sails together were not more than 7-8ths of the circular area containing them, yet a further addition rather diminished than increased the effect. *So that when the whole cylinder of wind is intercepted, it does not then produce the greatest effect for want of proper interslices to escape.*

It

It is certainly desirable, that the sails of windmills should be as short as possible ; but at the same time it is equally desirable, that the quantity of cloth should be the least that may be, to avoid damage by sudden squalls of wind. The best structure, therefore, for large mills, is that where the quantity of cloth is the greatest, in a given circle, that can be : on this condition, that the effect holds out in proportion to the quantity of cloth ; for otherwise the effect can be augmented in a given degree by a lesser increase of cloth upon a larger radius, than would be required, if the cloth was increased upon the same radius. The most useful figure therefore for practice, is that of N°. 9. or 10. as has been experienced upon several mills in large.

TABLE IV. Containing the Result of six Sets of Experiments, made for determining the Difference of Effect, according to the different Velocity of the Wind.

N. B. The sails were of the same size and kind as those of N° 10, 11, and 1: Tab. IV. Continuance of the Experiment one minute.

N°	Angle at the extremity.	Velocity of the wind in a second.	Turns of the sails unloaded.	Turns of the sails at maximum.	Load at the maximum.	Greatest load.	Product.	Maximum load for the half velocity.	Turns of the sails therewith.	Product of lesser load and greater velocity.	Ratio of the two products.	Ratio of the greatest velocity to the velocity at a maximum.	Ratio of the greatest load to the load at a maximum.
1	5°	4 4½	96	66	lb. 4,47	lb. 5,37	295	—	—	—	—	10:6,9	10:8,3
2	5	8 9	207	122	16,42	18,06	2003	4,47	180	805	10:27,3	10:5,9	10:9,1
3	7½	4 4½	—	65	4,62	—	300	—	—	—	—	—	—
4	7½	8 9	—	130	17,52	—	2278	4,62	180	832	10:27,8	—	—
5	10	4 4½	91	61	5,03	5,87	307	—	—	—	—	10:6,7	10:8,5
6	10	8 9	178	110	18,61	21,34	2047	5,03	158	795	10:26,	10:6,2	10:8,7
7	—	—	—	—	—	—	8	9	10	11	12	13	14

II. *Concerning the ratio between the velocity of windmill sails unloaded, and their velocity when loaded to a maximum.*

Those ratio's, as they turned out in experiments upon different kinds of sails, and with different inclinations (the velocity of the wind being the same) are contained in column 10 of tab. III. where the extremes differ from the ratio of $10 : 7,7$ to that of $10 : 5,8$; but *the most general ratio of the whole will be nearly as $3 : 2$* . This ratio also agrees sufficiently near with experiments where the velocity of the wind was different, as in those contained in tab. IV. col. 13. in which the ratio's differ from $10 : 6,9$ to that of $10 : 5,9$. However, it appears in general, that where the power is greater, whether by an enlargement of surface, or a greater velocity of the wind, that the second term of the ratio is less.

III. *Concerning the ratio between the greatest load that the sails will bear without stopping, or what is nearly the same thing, between the least load that will stop the sails, and the load at the maximum.*

Those ratio's for different kinds of sails and inclinations, are collected in col. 11. tab. III. where the extremes differ from the ratio of $10 : 6$ to that of $10 : 9,2$; but taking in those sets of experiments only, where the sails respectively answered best, *the ratio's will be confined between that of $10 : 8$ and of $10 : 9$; and at a medium about $10 : 8,3$ or of $6 : 5$* . This ratio also agrees nearly with those in col. 14 of tab. IV. However it appears, upon the whole, that in those instances, where the angle of the sails or

quantity of cloth were greatest, that the second term of the ratio was less.

IV. *Concerning the effects of sails, according to the different velocity of the wind.*

Maxim 1. *The velocity of windmill sails, whether unloaded, or loaded so as to produce a maximum, is nearly as the velocity of the wind, their shape and position being the same.*

This appears by comparing together the respective numbers of columns 4 and 5, tab. IV. wherein those of numbers 2, 4, and 6, ought to be double of numbers 1, 3, and 5: but as the deviation is no-where greater than what may be imputed to the inaccuracy of the experiments themselves, and hold good exactly in numbers 3 and 4; which setts were deduced from the medium of a number of experiments, carefully repeated the same day, and on that account are most to be depended upon; we may therefore conclude the maxim true.

Maxim 2. *The load at the maximum is nearly, but somewhat less than, as the square of the velocity of the wind, the shape and position of the sails being the same.*

This appears by comparing together the numbers in col. 6. tab. IV. wherein those of numbers 2, 4, and 6 (as the velocity is double), ought to be quadruple of those of numbers 1, 3, and 5; instead of which they fall short, number 2 by $\frac{1}{14}$, number 4 by $\frac{1}{19}$, and number 6 by $\frac{1}{13}$ part of the whole. The greatest of those deviations is not more considerable than might be imputed to the unavoidable errors

errors in making the experiments: but as those experiments, as well as those of the greatest load, all deviate the same way; and also coincide with some experiments communicated to me by Mr. Rouse upon the resistance of planes; I am led to suppose a small deviation, whereby the load falls short of the squares of the velocity; and since the experiments N^o 3 and 4. are most to be depended upon, we must conclude, that when the velocity is double, the load falls short of its due proportion by $\frac{1}{19}$, or, for the sake of a round number, by about $\frac{1}{20}$ part of the whole.

Maxim 3d. *The effects of the same sails at a maximum are nearly, but somewhat less than, as the cubes of the velocity of the wind.*

It has already been proved, Maxim 1st, that the velocity of sails at the *maximum*, is nearly as the velocity of the wind; and by Maxim 2d, that the load at the *maximum* is nearly as the square of the same velocity: if those two maximums would hold precisely, it would be a consequence that the effect would be in a triplicate ratio thereof: how this agrees with experiment will appear by comparing together the products in col. 8. of tab. 4. wherein those of N^o 2. 4. and 6. (the velocity of the wind being double) ought to be octuple of those of N^o 1. 3. and 5. instead of which they fall short, N^o 2. by $\frac{1}{7}$ N^o 4. by $\frac{1}{20}$, and N^o 6. by $\frac{1}{6}$ part of the whole. Now, if we rely on N^o 3. and 4. as the turns of the sails are as the velocity of the wind; and since the load of the maximum falls short of the square of the velocity by about $\frac{1}{20}$ part of the whole: the product

made by the multiplication of the turns into the load, must also fall short of the triplicate ratio by about $\frac{1}{10}$ part of the whole product.

Maxim 4th. *The load of the same sails at the maximum is nearly as the squares, and their effect as the cubes, of their number of turns in a given time.*

This maxim may be esteemed a consequence of the three preceding; for if the turns of the sails are as the velocity of the wind, whatever quantities are in any given ratio of the velocity of the wind, will be in the same given ratio of the turns of the sails: and therefore, if the load at the *maximum* is as the square, or the effect as the cube, of the velocity of the wind, wanting $\frac{1}{10}$ part when the velocity is double; the load at the *maximum* will also be as the square, and the effect as the cube, of the number of turns of the sails in a given time, wanting in like manner $\frac{1}{10}$ part when the number of turns are double in the same time. In the present case, if we compare the loads at the *maximum* col. 6. with the squares of the number of turns col. 5. of N^o 1 and 2. 5 and 6. or the products of the same numbers col. 8. with the cubes of the number of turns col. 5. instead of falling short, as N^o 3 and 4. they exceed those ratios: but as the sets of experiments N^o 1 and 2. 5 and 6. are not to be esteemed of equal authority with those of N^o 3 and 4. we must not rely upon them further than to observe, that *in comparing the gross effects of large machines, the direct proportion of the squares and cubes respectively, will hold as near as the effects themselves can be observed*; and there-
fore

fore be sufficient for practical estimation, without any allowance.

Maxim 5th. *When sails are loaded so as to produce a maximum at a given velocity, and the velocity of the wind increases, the load continuing the same; 1stly, The increase of effect, when the increase of the velocity of the wind is small will be nearly as the squares of those velocities: 2dly, When the velocity of the wind is double, the effects will be nearly as $10:27\frac{1}{2}$: But, 3dly, When the velocities compared, are more than double of that where the given load produces a maximum, the effects increase nearly in a simple ratio of the velocity of the wind.*

• It has already been proved, maxim 1st and 2d, that when the velocity of the wind is increased, the turns of the sails will increase in the same proportion, even when opposed by a load as the square of the velocity; and therefore if wanting the opposition of an increase of load, as the square of the velocity, the turns of the sails will again be increased in a simple ratio of the velocity of the wind on that account also; that is, the load continuing the same, the turns of the sails in a given time will be as the square of the velocity of the wind; and the effect, being in this case as the turns of the sails, will be as the square of the velocity of the wind also; but this must be understood only of the first increments of the velocity of the wind: for,

2dly, As the sails will never acquire above a given velocity in relation to the wind, tho' the load was diminished to nothing; when the load continues the same.

same, the more the velocity of the wind increases (tho' the effect will continue to increase) yet the more it will fall short of the square of the velocity of the wind; so that when the velocity of the wind is double, the increase of effect, instead of being as $1:4$, according to the squares, it turns out as $10:27\frac{1}{2}$, as thus appears. In tab. 4. col. 9. the loads of N^o 2, 4, and 6. are the same as the maximum loads in col. 6. of N^o 1, 3, and 5. The number of turns of the sails with those loads, when the velocity of the wind is double, are set down in col. 10. and the products of their multiplication in col. 11: those being compared with the products of N^o 1, 3, and 5. col. 8. furnish the ratios set down in col. 12. which at a medium (due regard being had to N^o 3 and 4.) will be nearly as $10:27\frac{1}{2}$. 3dly. The load continuing the same, grows more and more inconsiderable, respecting the power of the wind as it increases in velocity; so that the turns of the sails grow nearer and nearer a coincidence with their turns unloaded; that is, nearer and nearer to the simple ratio of the velocity of the wind. When the velocity of the wind is double, the turns of the sails, when loaded to a maximum, will be double also; but, *unloaded*, will be no more than triple, by deduction 2d: and therefore the product could not have increased beyond the ratio of $10:30$ (instead of $10:27\frac{1}{2}$) even supposing the sails not to have been retarded at all by carrying the maximum load for the half velocity. Hence we see, that when the velocity of the wind exceeds the double of that, where a constant load produces a maximum, that the increase of effect, which follows the increase of the velocity of the sails, will be nearly as the velocity

city of the wind, and ultimately in that ratio precisely. Hence also we see that windmills, such as the different species for raising water for drainage, &c. lose much of their full effect, when acting against one invariable opposition.

V. Concerning the effects of sails of different magnitudes, the structure and position being similar, and the velocity of the wind the same.

Maxim 6. *In sails of a similar figure and position, the number of turns in a given time will be reciprocally as the radius or length of the sail.*

The extreme bar having the same inclination to the plain of its motion, and to the wind; its velocity at a *maximum* will always be in a given ratio to the velocity of the wind; and therefore, whatever be the radius, the absolute velocity of the extremity of the sail will be the same: and this will hold good respecting any other bar, whose inclination is the same, at a proportionable distance from the center; it therefore follows, that the extremity of all similar sails, with the same wind, will have the same absolute velocity; and therefore take a space of time to perform one revolution in proportion to the radius; or, which is the same thing, the number of revolutions in the same given time, will be reciprocally as the length of the sail.

Maxim 7. *The load at a maximum that sails of a similar figure and position will overcome, at a given distance from the center of motion, will be as the cube of the radius.*

Geometry informs us, that in similar figures the surfaces are as the squares of their similar sides; of consequence the quantity of cloth will be as the square of the radius: also in similar figures and positions, the impulse of the wind, upon every similar section of the cloth, will be in proportion to the surface of that section; and consequently, the impulse of the wind upon the whole, will be as the surface of the whole: but as the distance of every similar section, from the center of motion, will be as the radius; the distance of the center of power of the whole, from the center of motion, will be as the radius also; that is, the lever by which the power acts, will be as the radius: as therefore the impulse of the wind, respecting the quantity of cloth, is as the square of the radius, and the lever, by which it acts, as the radius simply; it follows, that the load which the sails will overcome, at a given distance from the center, will be as the cube of the radius.

Maxim 8. The effect of sails of similar figure and position, are as the square of the radius.

By maxim 6. it is proved, that the number of revolutions made in a given time, are as the radius inversely. Under maxim 7. it appears, that the length of the lever, by which the power acts, is as the radius directly; therefore these equal and opposite ratios destroy one another: but as in similar figures the quantity of cloth is as the square of the radius, and the action of the wind is in proportion to the quantity of cloth, as also appears under maxim 7; it follows that the effect is as the square of the radius.

COROL.

COROL. 1. Hence it follows, that augmenting the length of the fail, without augmenting the quantity of cloth, does not increase the power; because what is gained by the length of the lever, is lost by the slowness of the rotation.

COROL. 2. If fails are increased in length, the breadth remaining the same, the effect will be as the radius.

VI. *Concerning the velocity of the extremities of windmill sails, in respect to the velocity of the wind.*

Maxim 9. *The velocity of the extremities of Dutch sails, as well as of the enlarged sails, in all their usual positions when unloaded, or even loaded to a maximum, are considerably quicker than the velocity of the wind.*

The *Dutch* fails unloaded, as in Tab. 3. No 8. made 120 revolutions in $52''$: the diameter of the fails being 3 feet 6 inches, the velocity of their extremities will be 25,4 feet in a second; but the velocity of the wind producing it, being 6 feet in the same time, we shall have $6:25,4::1:4,2$; in this case therefore, the velocity of their extremities was 4,2 times greater than that of the wind. In like manner, the relative velocity of the wind, to the extremities of the same fails, when loaded to a *maximum*, making then 93 turns in $52''$, will be found to be as $1:3,3$; or 3,3 times quicker than that of the wind.

The following table contains 6 examples of *Dutch* fails, and 4 examples of the enlarged fails, in different positions, but with the constant velocity of the wind of 6 feet in a second, from table 3 : and also 6 examples of *Dutch* fails in different positions, with different velocities of the wind, from table 4.

TABLE V. containing the ratio of the velocity of the extremities of windmill fails to the velocity of the wind.

N ^o	N ^o of Tab. III and IV.	Angle at the Extremity.	Velocity of the wind in a second.	Ratio of the velocity of the wind and extremities of the fails.	
				unloaded.	loaded.
1	8	0°	6 ^f 0 ⁱⁿ	1 : 4, 2	1 : 3, 3
2	9	3	6 0	1 : 4, 2	1 : 2, 8
3	10	5	6 0	— — —	1 : 2, 75
4	11	7½	6 0	1 : 4,	1 : 2, 7
5	12	10	6 0	1 : 3, 8	1 : 2, 6
6	13	12	6 0	1 : 3, 5	1 : 2, 3
7	14	7½	6 0	1 : 4, 3	1 : 2, 6
8	15	10	6 0	1 : 4, 1	1 : 2, 6
9	16	12	6 0	1 : 4,	1 : 2, 3
10	17	15	6 0	1 : 3, 35	1 : 2, 2
11	1	5	4 4½	1 : 4,	1 : 2, 8
12	2	5	8 9	1 : 4, 3	1 : 2, 6
13	3	7½	4 4½	— — —	1 : 2, 8
14	4	7½	8 9	— — —	1 : 2, 7
15	5	10	4 4½	1 : 3, 8	1 : 2, 6
16	6	10	8 9	1 : 3, 4	1 : 2, 3
1	2	3	4	5	6

From Table III.

From Tab. IV.

It appears from the preceding collection of examples, that when the extremities of the *Dutch* sails are parallel to the plane of motion, or at right angles to the wind, and to the axis, as they are made according to the common practice in *England*, that their velocity, unloaded, is above 4 times, and loaded to a *maximum*, above 3 times greater than that of the wind: but that when the *Dutch* sails, or enlarged sails, are in their best positions, their velocity unloaded is 4 times, and loaded to a *maximum*, at a medium the *Dutch* sails are 2,7, and the enlarged sails 2,6 times greater than the velocity of the wind. Hence we are furnished with a method of knowing the velocity of the wind, from observing the velocity of the windmill sails; for knowing the radius, and the number of turns in a minute, we shall have the velocity of the extremities; which, divided by the following divisors, will give the velocity of the wind.

Dutch sails in their common position	{ unloaded 4.2 loaded — 3.3
Dutch sails in their best position --	{ unloaded 4.0 loaded — 2.7
Enlarged sails in their best position	{ unloaded 4:0 loaded — 2.6

From the above divisors there arises the following compendiums; supposing the radius to be 30 feet, which is the most usual length in this country, and the mill to be loaded to a *maximum*, as is usually the case with corn mills; for every 3 turns in a minute, of the *Dutch* sails in their common position, the wind will move at the rate of 2 miles an hour; for every 5 turns in a minute, of the *Dutch* sails in their best position,

position, the wind moves 4 miles an hour; and for every 6 turns in a minute, of the enlarged sails in their best position, the wind will move 5 miles an

The following table, which was communicated to me by my friend Mr. Rouse, and which appears to have been constructed with great care, from a considerable number of facts and experiments, and which having relation to the subject of this article; I here insert it as he sent it to me: but at the same time must observe, that the evidence for those numbers where the velocity of the wind exceeds 50 miles an hour, do not seem of equal authority with those of 50 miles an hour and under. It is also to be observed, that the numbers in col. 3. are calculated according to the square of the velocity of the wind, which, in moderate velocities, from what has been before observed, will hold very nearly.

TABLE VI. *containing the velocity and force of wind, according to their common appellations.*

Velocity of the Wind.		Perpendicular force on one foot area in pounds avordupois.	Common appellations of the force of winds.
Miles in one Hour.	Feet in one second.		
1	1,47	,005	Hardly perceptible.
2	2,93	,020	} Just perceptible.
3	4,40	,044	
4	5,87	,079	} Gentle pleasant wind.
5	7,33	,123	
10	14,67	,492	} Pleasant brisk gale.
15	22,00	1,107	
20	29,34	1,968	} Very brisk.
25	36,67	3,075	
30	44,01	4,429	} High winds.
35	51,34	6,027	
40	58,68	7,873	} Very high.
45	66,01	9,963	
50	73,35	12,300	A storm or tempest.
60	88,02	17,715	A great storm.
80	117,36	31,490	An hurricane.
100	146,70	49,200	An hurricane that tears up trees, carries buildings before it, &c.
1	2	3	

VII. *concerning the absolute effect, produced by a given velocity of the wind, upon sails of a given magnitude and construction.*

It has been observed by practitioners, that in mills with Dutch sails in the common position, that when they make about 13 turns in a minute, they then
work

work at a mean rate: that is, by the compendiums in the last article, when the velocity of the wind is $8\frac{2}{3}$ miles an hour, or $12\frac{2}{3}$ feet in a second; which, in common phrase, would be called a *fresh gale*.

The experiments set down in Tab. IV. No 4. were tried with a wind, whose velocity was $8\frac{2}{3}$ feet in a second; consequently had those experiments been tried with a wind, whose velocity was $12\frac{2}{3}$ feet in a second, the effect, by maxim 3d, would have been 3 times greater; because the cube of $12\frac{2}{3}$ is 3 times greater than that of $8\frac{2}{3}$.

From Tab. IV. No 4. we find, that the sails, when the velocity of the wind was $8\frac{2}{3}$ feet in a second, made 130 revolutions in a minute, with a load of 17,25 lb. From the measures of the machine, preceding the specimen of a set of experiments, we find, that 20 revolutions of the sails raised the scale and weight 11,3 inches: 130 revolutions will therefore raise the scale 73,45 inches, which, multiplied by 17,52 lb, makes a product of 1287, for the effect of the Dutch sails in their best position; that is, when the velocity of the wind is $8\frac{2}{3}$ feet in a second: this product therefore multiplied by three, will give 3861 for the effect of the same sails, when the velocity of the wind is $12\frac{2}{3}$ feet in a second.

Desaguliers makes the utmost power of a man, when working so as to be able to hold it for some hours, to be equal to that of raising an hoghead of water 10 feet high in a minute. Now, an hoghead consisting of 63 ale gallons, being reduced into pounds averdupois, and the height into inches; the product made by multiplying those two numbers will be 76800; which is 19 times greater than the
pro-

product of the sails last-mentioned, at $12\frac{1}{2}$ feet in a second: therefore, by maxim 8th, if we multiply the square root of 19, that is 4,46, by 21 inches, the length of the sail producing the effect 3861, we shall have 93,66 inches, or 7 feet $9\frac{1}{2}$ inches for the radius of a Dutch sail in its best position, whose mean power shall be equal to that of a man: but if they are in their common position, their length must be increased in the ratio of the square root of 442 to that of 639, as thus appears;

The ratio of the *maximum* products of N° 8 and 11. Tab. III. are as 442:639; but by maxim 8, the effects of sails of different radii are as the square of the radii; consequently the the square roots of the products or effects, are as the radii simply; and therefore as the square root of 442 is to that of 639; so is 93,66 to 112,66; or 9 feet $4\frac{1}{2}$ inches.

If the sails are of the enlarged kind, then from Tab. III. N° 11 and 15. we shall have the square root of 820 to that of 639: 93,66:82,8 inches, or 6 feet $10\frac{1}{4}$ inches: so that in round numbers we shall have the radius of a sail, of a similar figure to their respective models, whose mean power shall be equal to that of a man;

The Dutch sails in their common position $9\frac{1}{2}$ feet.

The Dutch sails in their best position — 8

The enlarged sails in their best position — 7

Suppose now the radius of a sail to be 30 feet, and to be constructed upon the model of the enlarged sails, N° 14 or 15. Tab. III. dividing 30 by 7 we, shall have 4,28, the square of which is 18,3; and this, according to maxim 7, will be the relative

power of a sail of 30 feet, to one of 7 feet; that is, when working at a mean rate, the 30 feet sail will be equal to the power of 18,3 men, or of $3\frac{2}{3}$ horses, reckoning 5 men to a horse: whereas the effect of the common Dutch sails, of the same length, being less in the proportion of 820:442, will be scarce equal to the power of 10 men, or of 2 horses.

That these computations are not merely speculative, but will nearly hold good when applied to works in large, I have had an opportunity of verifying: for in a mill with the enlarged sails of 30 feet, applied to the crushing of rape seed, by means of two runners upon the edge, for making oil; I observed, that when the sails made 11 turns in a minute, in which case the velocity of the wind was about 13 feet in a second, according to article 6th, that the runners then made 7 turns in a minute: whereas 2 horses, applied to the same 2 runners, scarcely worked them at the rate of $3\frac{1}{2}$ turns in the same time. Lastly, with regard to the real superiority of the enlarged sails, above the Dutch sails as commonly made, it has sufficiently appeared, not only in those cases where they have been applied to new mills, but where they have been substituted in the place of the others.

VIII. *Concerning horizontal windmills and water-wheels, with oblique vanes.*

Observations upon the effects of common windmills with oblique vanes, have led many to imagine, that could the vanes be brought to receive the direct impulse, like a ship sailing before the wind, it would
be

be a very great improvement in point of power : while others attending to the extraordinary and even unexpected effects of oblique vanes, have been led to imagine, that oblique vanes applied to water-mills, would as much exceed the common water wheels, as the vertical windmills are found to have exceeded all attempts towards an horizontal one. Both these notions, but especially the first, have so plausible an appearance, that of late years there has seldom been wanting those, who have assiduously employed themselves to bring to bear designs of this kind : it may not therefore be unacceptable to endeavour to set this matter in a clear light.

PLATE VI. fig 2d. Let AB be the section of a plain, upon which let the wind blow in the direction CD , with such a velocity as to describe a given space BE , in a given time (suppose 1 second); and let AB be moved parallel to itself, in the direction CD . Now, if the plane AB moves with the same velocity as the wind; that is, if the point B moves thro' the space BE in the same time that a particle of air would move thro' the same space; it is plain that, in this case, there can be no pressure or impulse of the wind upon the plane : but if the plane moves slower than the wind, in the same direction, so that the point B may move to F , while a particle of air, setting out from B at the same instant, would move to E , then BF will express the velocity of the plane; and the relative velocity of the wind and plane will be expressed by the line FE . Let the ratio of FE to BE be given (suppose $2 : 3$.); let the line AB represent the impulse of the wind upon the plane AB , when acting with its whole velocity BE ; but,

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when acting with its relative velocity FE , let its impulse be denoted by some aliquot part of AB , as for instance $\frac{4}{9} AB$: then will $\frac{4}{9}$ of the parallelogram AF , represent the mechanical power of the plane; that is, $\frac{4}{9} AB \times \frac{1}{3} BE$.

2dly, Let IN be the section of a plane, inclined in such a manner, that the base IK of the rectangle triangle IKN may be equal to AB ; and the perpendicular $NK=BE$; let the plane IN be struck by the wind, in the direction LM , perpendicular to IK : then, according to the known rules of oblique forces, the impulse of the wind upon the plain IN , tending to move it according to the direction LM , or NK , will be denoted by the base IK ; and that part of the impulse, tending to move it according to the direction IK , will be expressed by the perpendicular NK . Let the plane IN be moveable in the direction of IK only; that is, the point I in the direction of IK , and the point N in the direction NQ , parallel thereto. Now it is evident, that if the point I moves thro' the line IK , while a particle of air, setting forwards at the same time from the point N , moves thro' the line NK , they will both arrive at the point K at the same time; and consequently, in this case also, there can be no pressure or impulse of the particle of the air upon the plane IN . Now let IO be to IK as BF to BE ; and let the plane IN move at such a rate, that the point I may arrive at O , and acquire the position OQ , in the same time that a particle of wind would move thro' the space NK : as OQ is parallel to IN ; (by the properties of similar triangles) it will cut NK in the point P , in such a manner, that $NP=BF$, and $PK=FE$: hence it appears,

appears, that the plane IN , by acquiring the position OQ , withdraws itself from the action of the wind, by the same space NP , that the plane AB does by acquiring the position FG ; and consequently, from the equality of PK to FE , the relative impulse of the wind PK , upon the plane OQ , will be equal to the relative impulse of the wind FE , upon the plane FG : and since the impulse of the wind upon AB , with the relative velocity FE , in the direction BE , is represented by $\frac{4}{9} AB$; the relative impulse of the wind upon the plane IN , in the direction NK , will in like manner be represented by $\frac{4}{9} IK$; and the impulse of the wind upon the plane IN , with the relative velocity PK , in the direction IK , will be represented by $\frac{4}{9} NK$: and consequently the mechanical power of the plane IN , in the direction IK , will be $\frac{4}{9}$ the parallelogram IQ : that is $\frac{1}{3} IK \times \frac{4}{9} NK$: that is, from the equality of $IK=AB$ and $NK=BE$, we shall have $\frac{4}{9} IQ = \frac{1}{3} AB \times \frac{4}{9} BE = \frac{4}{9} AB \times \frac{1}{3} BE = \frac{4}{9}$ of the area of the parallelogram AF . Hence we deduce this

GENERAL PROPOSITION,

That all planes, however situated, that intercept the same section of the wind, and having the same relative velocity, in regard to the wind, when reduced into the same direction, have equal powers to produce mechanical effects.

For what is lost by the obliquity of the impulse, is gained by the velocity of the motion.

Hence it appears, that an oblique sail is under no disadvantage in respect of power, compared with a direct one; except what arises from a diminution of

its breadth, in respect to the section of the wind: the breadth *IN* being by obliquity reduced to *IK*.

The disadvantage of horizontal windmills therefore does not consist in this; that each sail, when directly exposed to the wind, is capable of a less power, than an oblique one of the same dimensions; but that in an horizontal windmill, little more than one sail can be acting at once: whereas in the common windmill, all the four act together: and therefore, supposing each vane of an horizontal windmill, of the same dimensions as each vane of the vertical, it is manifest the power of a vertical mill with four sails, will be four times greater than the power of the horizontal one, let its number of vanes be what it will: this disadvantage arises from the nature of the thing; but if we consider the further disadvantage, that arises from the difficulty of getting the sails back again against the wind, &c. we need not wonder if this kind of mill is in reality found to have not above $\frac{1}{8}$ or $\frac{1}{10}$ of the power of the common sort; as has appeared in some attempts of this kind.

In like manner, as little improvement is to be expected from water-mills with oblique vanes: for the power of the same section of a stream of water, is not greater when acting upon an oblique vane, than when acting upon a direct one: and any advantage that can be made by intercepting a greater section, which sometimes may be done in the case of an open river, will be counterbalanced by the superior resistance, that such vanes would meet with by moving at right angles to the current: whereas the common floats always move with the water nearly in the same direction.

Here

Here it may reasonably be asked, that since our geometrical demonstration is general, and proves, that one angle of obliquity is as good as another; why in our experiments it appears, that there is a certain angle which is to be preferred to all the rest? It is to be observed, that if the breadth of the sail IN is given, the greater the angle KIN , and the less will be the base IK : that is, the section of wind intercepted, will be less: on the other hand, the more acute the angle KIN , the less will be the perpendicular KN : that is, the impulse of the wind, in the direction IK being less, and the velocity of the sail greater; the resistance of the medium will be greater also. Hence therefore, as there is a diminution of the section of the wind intercepted on one hand, and an increase of resistance on the other, there is some angle, where the disadvantage arising from these causes upon the whole is the least of all; but as the disadvantage arising from resistance is more of a physical than geometrical consideration, the true angle will best be assigned by experiment.

SCHOLIUM.

In trying the experiments contained in Tab. III. and IV. the different specific gravity of the air, which is undoubtedly different at different times, will cause a difference in the load, proportional to the difference of its specific gravity, tho' its velocity remains the same; and a variation of specific gravity may arise not only from a variation of the weight of the whole column, but also by the difference of heat of the air concerned in the experiment, and possibly of other causes; yet the irregularities that might arise from a dif-

difference of specific gravity were thought to be too small to be perceivable, till after the principal experiments were made, and their effects compared; from which, as well as succeeding experiments, those variations were found to be capable of producing a sensible, tho' no very considerable effect: however, as all the experiments were tried in the summer season, in the day-time, and under cover; we may suppose that the principal source of error would arise from the different weight of the column of the atmosphere at different times; but as this seldom varies above $\frac{1}{17}$ part of the whole, we may conclude, that tho' many of the irregularities contained in the experiments referred to in the foregoing essay, might arise from this cause; yet as all the principal conclusions are drawn from the medium of a considerable number, many whereof were made at different times, it is presumed that they will nearly agree with the truth, and be altogether sufficient for regulating the practical construction of those kind of machines, for which use they were principally intended.

XIX. *An Account of the remarkable Alteration of Colour in a Negro Woman : In a Letter to the Reverend Mr. Alexander Williamson of Maryland, from Mr. James Bate, Surgeon in that Province. Communicated by Alexander Ruffel, M. D. F. R. S.*

To the Rev. Tho. Birch, D. D. Secret. R. S.

S I R,

Read, 10 May, 1759. **S**OME account of the extraordinary facts mentioned in the inclosed letter having been communicated to me, above a year ago, by the Reverend and ingenious Mr. Williamson of Maryland, I thought them worthy of attention; and therefore begged, that he would be so kind, as to get some Gentlemen of the faculty to assist him in making a further enquiry. He has accordingly transmitted to me the case, now sent you, as related by Mr. Bate, a practitioner in physic, of some note in that part of the world.

I have also had the particulars since confirmed to me by two Gentlemen now in England, who have often seen the woman; so shall make no farther apology for giving you this trouble, in order to its being communicated to the Royal Society.

I am, Sir,

Your most obedient humble Servant,

Limestreet, May 8th,
1759.

Alexander Ruffel.

S I R,

S I R,

IN compliance with your desire, I send as particular an account of the extraordinary metamorphosis observable in colonel Barnes's negro woman, as I have been able to procure.

Frank, a cook-maid of the above-named Gentleman, a native of Virginia, about 40 years of age, remarkably healthy, of a strong and robust constitution, had her skin originally as dark as that of the most swarthy African; but about fifteen years ago, observed that membrane, in the parts next adjoining to the finger nails, to become white. Her mouth soon underwent the same change, and the phenomenon hath since continued gradually to extend itself over the whole body; so that every part of its surface is become more or less the subject of this surprising alteration. In her present state four parts in five of the skin are white, smooth, and transparent, as in a fair European, elegantly shewing the ramifications of the subjacent blood-vessels: the parts remaining sooty daily lose their blackness, and in some measure partake of the prevailing colour; so that a very few years will, in all probability, induce a total change. The neck and back, along the course of the vertebræ, maintain their pristine hue the most, and in some spots proclaim their original state: the head, face, and breast, with belly, legs, arms, and thighs, are almost wholly white; the pudenda and axillæ partly-coloured; the skin of these parts, as far as white, being covered with white hair; where dark, with black. Her face and breast, as often as the passions anger, shame, &c. have been excited.

excited in her, have been immediately observed to glow with blushes; as also when, in pursuance of her business, she has been exposed to the action of the fire upon these parts, some freckles have made their appearance. After having described her present appearance, as well as I am able, I shall not pretend to offer any conjectures of my own upon the subject; lest being led away by a train of reasoning, I should lose myself, in endeavouring to establish a favourite hypothesis; but, on the contrary, shall confine myself to a simple narration of such facts, as may prevent mistakes, or obviate difficulties, arising in the investigation of this difficult piece of physical history. And in the first place, lest the change should be thought the consequence of a previous morbid state, she declares, that, excepting about seventeen years ago, when she was delivered of a child, she hath never been afflicted by any complaint of 24 hours continuance; and that she never remembers the catamenia to have been either irregular or obstructed, only during this pregnancy: she hath never been subject to any cutaneous disorders, or made use of any external applications, by which this phenomenon might be produced. The effects of the bile upon the skin are well known to physicians, and have given rise to an opinion, that its colour was determined thereby: for my own part I cannot believe it has any thing to do here, since, from all the circumstances I have been able to collect, I cannot find the least reason to suspect, that this fluid, whether cystic or hepatic, has undergone any alteration. As unction is known to make the skin of negroes become white, and as she is daily employed in the businesses

of cookery, it may perhaps be supposed the effect of heat : but this can never be the case, as she has ever been well clad ; and the change is as obvious in the parts protected from the action of that element, as in those the most exposed thereto. As an emunctory, the skin seems to perform its office as well as possible, the sweat with the greatest freedom indifferently pervading the black and white parts. The effects of a blister I mentioned to you I am yet a stranger to, as that, which I applied upon the outside of the arm, did not answer the intended purpose : whether this was owing to its being laid upon a part too much exposed, or that the corpus reticulare being destroyed, there may be such an adhesion of the cuticle to the cutis, as may render them inseparable, a second experiment must determine. If, upon your sending this to Dr. Russell, he, or any of his learned acquaintance, to whom he may communicate it, shall think any future experiments necessary, I shall be glad to execute them under their directions, not only for my private satisfaction, but in order to convince you, how much pleasure I take in doing every thing, that may oblige Mr. Williamson or his friends.

I am, Sir,

Your obedient humble Servant,

Leonard-Town,
6th Sept. 1758.

James Bate,
Surgeon in Maryland:

XX. *The Case of a paralytic Patient cured by an electrical Application, inclosed in a Letter from Doctor Himfel, at Riga, to Jacob de Castro Sarmiento, M. D. F. R. S. Translated from the French.*

Read May 17,
1759.

ON the 10th of March 1752, a young man 20 years of age, whose name is *Mauve*, in his infancy had a fall, by which his right arm became paralytic, of which he never had the least use from five years old. He was brought to me in order to attempt his cure by electricity; and he was in much the same condition with the patient cured by Mr. *Jalabert* at Geneva; who could not move his right hand in the least, and was afterwards able to help himself, and follow his trade. All the fingers of the paralytic hand were disabled, and the hand was so bent towards the elbow, as to form a right angle; the hand was of a red and blue colour, as if it had been struck with frost. The extensor muscles of the wrist and fingers had an *atrophy*, and the whole fore-arm was shrivled; besides which, all his right side was paralytic, as well as the right arm, at the beginning of his disease; and the right foot was very much weakened.

He approached the electrical tube, and touched it for some minutes; till the *thenar*, *hypothenar*, *anti-thenar*, *indicator*, and the *interossei*, as well as the fingers of the lame hand, suffered electrical shocks one after another.

The spectators were amazed to find, that, at this first trial, the young man could successively extend his *thumb*, *index*, and *middle fingers*, and at length the ring and little fingers ; and on the following days his friends observed, that his hand was no longer so rigid as it had been. The good effect of this first essay, and the desire of the patient and his relations, encouraged me to repeat it on the 16th ; and I increased the electrical power by the known means of the flagon of water. I stripped his arm, and by turns gave the shock to the *cubitæus externus*, *radius externus*, the *extensor magnus*, the *interossei*, *extensor pollicis*, and the other muscles of the thumb. He complained sometimes, that his hand and arm were numbed and stiff : I therefore caused it to be well rubbed with a towel, by which it became flexible : I made them also rub his hand with a woollen cloth, and continue it some time ; and I observed, that this hand, which bent towards the elbow for fifteen years, became straightened out, altho' it fell into its former situation some seconds after. This was the more remarkable, as many applications had been made before by the ablest physicians. The 17th the patient told me, that his hand opened of itself the day before at three different times, and was also straightened out with the elbow ; but that it closed and bent up again of itself. The second time that I electrified him, he was more sensible of the sparks, and this day he felt them still more, having made them stronger by Muschenbroek's invention ; he could scarce bear them : the numbness of his arm and hand followed as before, which generally happened afterwards upon electrifying him ; but he always found him-

himself better upon rubbing, as before. This third time he extended his hand and fingers more than once, without the assistance of his other hand : nevertheless, as he complained of very sharp blows from the sparks, I changed my first method the 18th, and mounting upon a box filled with pitch, I took the tube in one hand, and, by means of a key which I held in the other, I touched him where I thought proper, in order to draw the sparks ; but as the shocks were very strong, I placed under the tube two flagons filled with water, making a communication of the water with the tube, by means of an iron wire. In viewing the naked arm, it plainly appeared it was become more fleshy. The night after he slept better than the foregoing between the 17th and 18th, when, tho' he found himself much fatigued all over, his sleep was often interrupted by very sharp pains in the upper part of his arm ; which part, nevertheless, I had not touched at all ; but he had that day undergone very strong shocks. He also felt great heat in his hand together with these pains, and this heat was felt almost every time after he had been electrified, both in the hand and the whole length of the arm.

On the 19th, 20th, and 21st, some affairs prevented my continuing the operation ; but I advised him to exercise his arm every way he possibly could, having observed, that even the muscles of the arm least affected, after so long a time of inactivity, were much weakened ; and the patient was gradually capable of taking up his glove from the ground several times successively, and even to put on and take off his hat with the paralytic hand, which he also repeated the following days. On the 22d, I observed, upon
the

the upper part of the arm, near the *deltoid* muscle (which was still very large near the *biceps*) and the *extensores cubiti*, two deep hollows; and the extension of the elbow was made with great difficulty: wherefore I touched him chiefly upon those muscles which cover the upper part of the arm, having increased the electrical power in Muschenbroek's manner; for he seemed to come on but slowly while the shocks were but mild. But I made the necessary dispositions to hinder his being at the trouble of lifting up his arm to touch the tube; I applied it to the hand and arm where I judged necessary; and, on the 23d following, he was able to lift and carry a weight of sixteen pounds and an half.

I repeated the operation on the 23d, 24th, and 25th. He was extremely sensible of the slightest shocks on the last day, so as to excite compassion in all that were present. Besides which, during the time that his arm was electrifying, I observed certain protuberances in those places, from whence I drew the sparks, like those which professor Jallabert had seen in his patient; but in these two days they became very large, and, upon rubbing the arm, the skin peeled off: notwithstanding they diminished on the following days, altho' the shocks were made more powerful; which is the more remarkable. The *deltoid* muscle, which, on the 22d, was observed to be so large, became much less; and the hollows, which were between this and the *biceps* and *extensores* of the elbow, were filled up; but, upon bending the elbow, there remained still a preternatural rigidity.

On the 27th, the patient gave several proofs of the advantage he had gained by the electricity, in the pre-

fence of several persons: he opened and shut the fingers of his right hand without the assistance of the other: he could stretch out and bend the *carpus* and *metacarpus* at pleasure: he took up from the ground his glove and other things, and a weight of $16\frac{1}{2}$ lb. above three feet, moving it backwards and forwards at the same time; and he could have employed more strength with the affected arm, than was sufficient to hold up the weight, without any inconvenience.

We then stripped both arms, and found, that, as to the external appearance, the paralytic arm was become more like the other; and the hand and fingers were better covered with flesh than before. Besides, several muscles of the arm, especially those of the fore-arm, were fuller; the blue and red colour of that hand disappeared, and it was now like the other. But the extension of the elbow was yet a little difficult; nor were the fingers yet sufficiently flexible; and therefore it was somewhat troublesome for him to lay hold on, and keep, any thing in his hand. On the 28th, he performed all those proofs before some professors and doctors of the academy with success. They viewed his arms and hands, and the change that was brought about was evident. I afterwards electrified his arm and hand; but principally the extensors of the elbow, the great extensor, and the interosseous muscles, and also the *indicator* and muscles of the thumb. The 29th he told me, that the arm sweated continually from the day before, which happened several times after being electrified; and that the sweat would often continue till next day. On repeating, this day, my operation, he

he sweated all over his body; and having returned home, he felt as if a number of globules of blood flowed up his affected arm, which made so strong an impression upon him, that it frightened him; but having stripped his arm, no sort of alteration appeared.

On the 31st of March, he was able to take up a glass of beer in the paralytic hand, to hold it steady, and put it to his mouth, drinking to the health of all the company one after another; and since that time he helps himself at table with his right hand. When he came to me on the first of April, he lifted from the ground, almost three feet, a weight of 33 lb. in the presence of several persons, which he was not able to do before, tho' he had tried several times. On this day the arm was electrified again in the parts that required; but as the sensation in this arm was nearly equal to that of the other, he was no longer able to undergo the shocks, for an hour, as he used to do; tho' he was very sensible of the advantages gained by the electrical operation: however, he fainted away, and therefore we were forced to forbear a little. Besides this, he was often subject to a looseness; which Noguez at Geneva was also. On the 4th of April in the evening, I exposed the paralytic arm once more to the electrical sparks; and as there always were some persons by at the operations, there was this time a Frenchman present, who had a megrim. He underwent the shock twice, according to Muschenbroek's method; and came to thank me next day for having cured the disorder of his head. On the 5th of April, I again electrified my patient, and the sensation was now nearly recovered in the paralytic arm, which was restored to a healthy condition. He extended his

his fingers, and contracted them at pleasure, could move the *carpus* and *metacarpus* at will; he took off his hat, and put it on, and had gained so much strength in his hand and arm, that he raised a forty pound weight to the height of three feet from the ground.

Such was the state of my patient after having electrified him fourteen times from the 10th of March, almost an hour each time. I did not touch his foot, which was a little paralytic from the beginning, having never been very troublesome to him.

From the 5th to the 27th of April, he was electrified eleven times more; during which time the strength of his arm still increased: he not only raised above 40 lb. weight with his right hand, moving it backwards and forwards at the same time, but he wrote his name, *Andrew Mauve*, with a craion, with the same hand, which he had not been capable of moving for fifteen years before.

John Godfrey Teske.

XXI. *An Account of some Observations relating to the Production of the Terra Tripolitana, or Tripoli. Humbly addressed to the Royal Society of London, by Martin Hubner, Fellow of the said Society, Professor of History in the University of Copenhagen, and Member of the Royal Academy of Inscriptions and Belles Lettres of Paris. Translated from the French, by Emanuel Mendes da Costa, F. R. S.*

Read June 21,
1759.

DURING a journey I made, in the autumn of the year 1755, through several provinces of France, principally in Britany, I made the natural history of that province, which has plenty of productions worthy the attention of a naturalist, one of the objects of my researches. The lead mines of Poullaüen in the Lower, and those of Pontpean in the Upper Britany, employed me some time. The metallic veins in these mines are not only rich and regular, but also hold a great proportion of silver, which they do not extract with that profit, that might be done. In general, they could work these mines to greater advantage, if they were more skillful, or had proper machines, and above all others the fire engine, which is used with such success in the mines of Cornwall to drain off the water, that element being the greatest obstacle to the right working of the mines of Britany: but the advantages of this invention, glorious to its first discoverer, and useful
to

to this nation, where it has been perfected, are yet so unknown in France, that there are even mechanicians in that kingdom, who seriously doubt, whether the fire engine is any-wise useful. I shall not here mention any thing of some rare and curious fossils found in Britany, nor of the square stones of a particular species, on the formation of which the late M. de Robier, president of the parliament of Rennes, who had a magnificent collection of natural history, employed his thoughts. I likewise shall pass over in silence the marbles and the plumb pudding stones, called in France *cailoux de Rennes*, from the vast quantities of them found in the neighbourhood of that city. The chief and only subject I propose in this dissertation, is the generation or production of the terra Tripolitana, or Tripoli, of which there are great quantities, and of the best kind, in Upper Britany. I more readily determine to give this illustrious Society an account of my observations on this subject, as not only the discovery I think I have made thereon is very curious, but also that the generation of this earth has been hitherto utterly unknown, no one having, to my knowlege, explained, before me, in what manner it is produced. It is true, I heard in France, that a young gentleman, a native of Britany, had wrote somewhat on this subject, and that his dissertation was to be inserted in a collection of miscellaneous papers, or loose pieces; but I never could see his said dissertation, nor know what it contained; therefore I am incapable of judging what it is.

In the mountain de Poligné, called by the Bretons *le Tertre gris*, i. e. the grey hill, I think I have discovered the true origin of Tripoli. This mountain,

the Breton name whereof is manifestly derived from the greyish colour of its summit, is situated in Upper Brittany, near the inn of Roudun, on the road from Rennes to Nantes, a small league wide of Barus, and five leagues from Rennes. Observations and experience are the only means, that can conduct us to certain knowledge in natural history; and it is conformable to that axiom, that I mean, simply, to lay before the Society what I observed in that mountain, relating to the generation of Tripoli, and, at the same time, to produce also the most authentic documents of nature, in order to prove, that the terra Tripolitana, or Tripoli, is probably only *a wood wholly petrified, and afterwards calcined by the subterraneous fire.*

To give more weight to what I propose, and to establish this assertion, it will be necessary to observe here, that the mountain of Poligné, in the interior of which the Tripoli is found, has been, and is perhaps yet, a volcano. Its colour, its form, its fissures, and its strata, prove it; and the inhabitants of the neighbourhood declare, that they have formerly seen fire on its summit at night; but that, however, for many years past, they have not perceived any more. This being granted, I have only now to give an account of what is to be observed in the mountain itself, and to produce specimina of the different strata of earth found therein, to ascertain the truth of my thesis.

The stratum of the true Tripoli, intirely calcined, lies from 50 to 60 feet depth; the sample of it here produced is marked N^o 1. This stratum is white; but it sometimes has a cast of grey, and sometimes is of a reddish hue, as the sample itself shews. It is quite or fully calcined and converted into Tripoli, because it
lay

lay near to the subterranean fire of the volcano, so as to be violently affected by it.

The stratum above this, of which N^o 2. is a sample, is already much burnt and calcined, but not enough to be whitened, having lain too far from the violence of the subterraneous fire to be freed intirely from its heterogeneous and combustible parts, inso-much that the fire has left it quite black.

The small layer or stratum, which follows, that is, lies above it, or overlays the last, and of which N^o 3. is a small sample, is yet yellowish, verging on brown; and altho' the burning is easily seen on the extremities, and the effect of the fire in the interior part of the layer, yet it is easily to be understood, that it is the weakness of the fire incapable of reaching it with a force necessary to calcine it, that has put it and left it in this state of imperfection. When one views this sample narrowly, vestiges, which appear pores of a wood, are easily discovered.

The next layer shews the same thing, but more clearly: the pieces marked N^o 4. are samples of it. This layer has been less attacked by the fire than those under it; nevertheless the lightness of the substance shews, that it is already a little calcined, and the sight alone clearly demonstrates the resemblance of its pores to those of wood.

The piece taken from the fifth stratum, and marked N^o 5. confirms this same observation. It is more weighty in proportion than all the others, because it is less calcined; and indeed it is less calcined, only because it was the farthest distant from the subterranean fire, and thereby has suffered less, as I have observed before. However, its ends or extremities evi-

dently prove the action of the fire even on this higher or upper stratum ; and its interior parts likewise prove , as evidently, that this substance has been formerly petrified wood, but from which the ligneous parts have been driven away, and consumed by a heat superior to their resistance.

I do not doubt, but had my time permitted me to make further researches, I should have found in the mountain pieces of petrified wood not yet altered or destroyed by the action of the fire ; but my leisure not answering to my wishes, I thought I might remain contented with what I have now the honour to present to the Society, and which seems to me sufficient to determine the process of nature in the generation or production of Tripoli.

If I am asked how all this vast quantity of wood could be heaped or gathered together in this mountain ? I answer, 1^o. That it is not to be supposed that the several layers or strata of Tripoli, perfect or imperfect, follow each other without any interruption. 2^{dly}, It may be allowed me to suppose a deluge, whatever it was, and which has covered many parts of our globe, to accumulate here all this wood, and even aid its petrification. 3^{dly}, A further proof of my assertion is the wood-coals *, which incontestably are found deep in the earth ; for there are intire quarries of them in Saxony, in the neighbourhood of Halle, and which do not suppose a less quantity of wood : besides, it is useless to dispute against observation and experience, when they are solidly established.

* Wood-coals. The author uses the words " charbons de bois ;" but what he means by his intire quarries of charbons de bois, I declare myself ignorant of.

I must here further observe, that the volcano of Poligné did not, perhaps, cease to flame on the upper part of the mountain, till after the ligneous parts of the wood buried in it ceased to furnish the pabulum necessary to the fire.

I shall be happy, if these observations merit the approbation of this illustrious Society, to whom I have the honour to dedicate them ; but, however, am delighted, that they procure me an occasion of testifying my zeal for this respectable and learned body, in communicating to it all observations, which appear to me useful.

However, I do not absolutely pretend, that all Tripoli is a wood, wholly or *per totum* petrified, and afterwards calcined by the subterraneous fire. There are Tripolis of many kinds, as well as of several degrees of goodness ; and among these there may be such, as are not otherwise than a real or native fossil, deprived by fire of its primitive hardness and weight : but yet I believe, that the Tripoli, which has its origin from a petrified wood, must be the best, because naturally it ought to be the finest, softest, and the best calcined.

London, 8th Octob. 1757.

Remarks on Mr. Hubner's Paper on Tripoli.

HE is of opinion, that the different strata, marked in his specimina N^o 1, 2, 3, 4, and 5. are all the same, only under different degrees of calcination. But N^o 2. seems to differ from the rest, as it looks like mere charcoal, and appears to be very little, if
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at all, petrified or saturated with extraneous particles ; I would therefore propose, not only that this N^o 2. should be further calcined, in order to be satisfied, whether it will, by that means, come to be of the same nature with N^o 1. but likewise that the following N^o 3, 4, and 5. which Mr. Hubner supposes to be less calcined, should undergo the same trial, in order to observe, whether they will by that means become Tripoli ; also to know, whether by being calcined for some time (before they are reduced to tripoli) they will put on the appearance of a coal like N^o 2. for, if they do not, as I suspect may be the case, it will be little less than a proof, that N^o 2. which seems the plainest wood of any, differs from the others more essentially than merely in its degree of calcination. Perhaps it may also be found, that N^o 3, 4, and 5. differ not only from N^o 2. but likewise from N^o 1. and may not be capable of being reduced to Tripoli.

Remarks on the preceding Paper : In a Letter to the Right Honourable the Earl of Macclesfield, Pres. R. S. from Mr. Emanuel Mendes da Costa, F.R.S.

My Lord,

Read June 21, 1759. **Y**OUR Lordship's commands to wait on Mr. Professor Hubner, when in England, to receive his paper on the production of Tripoli, which he designed for the Royal Society, and to discourse with him thereon, in order to translate it, and

lay it before this illustrious body, I accordingly obeyed; and the inclosed is the said paper. My avocations, which now absorb many hours I formerly dedicated to study, have been the only cause, my Lord, that I have detained it so long; I therefore hope for your Lordship's and the Society's pardon.

When I first undertook the translation, I had thoughts of giving my opinion thereon, not only by reasoning, but also by experiments. Time, my Lord, has not permitted me to do so; and since, having seriously reflected, that Mr. Hubner, in his last paragraph, turns his intire system into a partial production of one species of Tripoli, I think it unnecessary to trouble your Lordship, or the Society, with any arguments pro or con. I shall only observe, that it is not improbable but some of Mr. Hubner's Tripoli, as he surmises, may have been produced from the petrified wood he found in the mountain; and the whole account is then reduced to this only circumstance, that the layers of fossil wood in this mountain, having been saturated with the Tripoline particles, which likewise abound in the same mountain, thereby composed a stone, or third body; and that afterwards these Tripoline particles were again reduced, by the effects of a subterraneous fire, to their pristine state; the force of the fire destroying the compages of the third body, or stone.

Had the wood, my Lord, been saturated with any other metallic mineral, or earthy particles, I believe every judge of science will determine, that the calcination of petrified wood, alone, could never have changed it into Tripoli.

I have, in my history of fossils, p. 76. 85. and 87. described five kinds of the Tripoli earth; and Mr.

Hubner's kind, here mentioned, is only a variety of that I call *creta*, *tripela alba dicta*, p. 76. N° 1. Your Lordship will please to observe, that of all the said kinds none are produced in general by any such operations of nature, as Mr. Hubner intimates; therefore, my Lord, reason will convince, that this is only a partial and local origin of his Tripoli, by concurring circumstances of wood and Tripoli buried together in the bowels of a volcano; for as we find this very species elsewhere produced without such circumstances, it is certain they are not the sole efficient causes of its production.

I am, with great submission and respect,

My Lord,

Your Lordship's most devoted,

most obliged, and most obedient

humble servant,

Bearbinder Lane,
19 June, 1759.

Emanuel Mendes da Costa.

XXII. *A remarkable Case of an Empyema.*
By Mr. Joseph Warner, F. R. S. and Surgeon to Guy's Hospital.

Read June 28, 1759. **M**MORRIS EVANS, aged 30 years, on the 13th of March, 1759, was admitted into Guy's hospital, with a remarkable complaint in his chest, which attacked him in the month of August 1758, with the symptoms of a pleurisy.
Upon

Upon inspection it appeared, that the left side of the thorax was greatly enlarged, and prodigiously distended: the pectoral muscle was somewhat raised up; on pressure it felt soft, and readily gave way: upon a removal of the pressure, the integuments resumed their former appearance, no marks of impression remaining on this, or any other part of the thorax, so as to constitute the characteristic of an œdematous swelling.

The spaces betwixt the 9th and 10th, and betwixt the 10th and 11th ribs, counting from above, were visibly enlarged, and somewhat elevated: they felt soft, and yielded to the fingers; but were not at all inflamed, or otherwise discoloured. Upon examination, I discovered a fluctuation in both these parts. The general symptoms that attended this case were similar to those arising from all considerable collections of fluids deposited in either cavities of the thorax: for instance, the patient had a continual flow fever; a short cough, but without the least expectoration of matter; a great difficulty in respiration, particularly in the acts of expiration. He was incapable of lying down on the right side, without very great uneasiness; he was much emaciated; and his countenance was uniformly fallow: he did not complain of so much pain, or so great a difficulty in breathing, when in an erect posture, as I have sometimes observed in diseases of this kind even where the quantity of extravasated fluid has been much less; but at the same time I must acknowledge, that no fair inference could be deduced from hence, because of the peculiar position the diseased side was put in when the poor man sat down, or stood up;

either of which he was incapable of doing without being supported. The left side of the thorax inclined forwards, and protuberated in a peculiar manner, so as to give the head and trunk an horizontal posture; in which position of the body, the weight of the contained fluid most certainly was, in part, prevented from pressing so forcibly upon the left portion of the diaphragm, the mediastinum, and the right portion of the lungs, as it must necessarily have done in a more erect position of the body. He had one symptom, which I had never before observed in patients labouring under this complaint; that is, he was incapable of lying on his back, without bringing on very alarming threats of suffocation; but he did not remember ever to have heard any noise or rattling of the pus upon motion. He could lie most conveniently on his left side; but even that posture was of late become very painful to him. In short, he could find no tolerable posture to put his body into, but that of inclining it considerably forwards, which (I have already observed) he was under a necessity of doing, to enable him to draw his breath; and I dare venture to say, that, upon attending to the subsequent part of the history of this poor mortal's case, the reason, why such effects should be produced from such a cause, will very readily occur to those, who have a moderate degree of knowledge of the formation and uses of these parts of the human body. Upon making an incision upon the most prominent part of the space betwixt the 10th and 11th rib, in the cavity of the thorax of the left side, at least eight Winchester quarts of a thin yellow matter, not at all foetid, was discharged upon the spot in a full stream: the
matter

matter issued thro' the wound by leaps, and was projected at the distance of two yards and upwards from the patient's body: he did not faint during the operation, nor afterwards; but from that moment he grew easy; his symptoms abated; he slept well at night; and the next day he had no bad symptoms, except that his difficulty in breathing was not quite removed. This, indeed, could not reasonably be expected, considering the long disuse and extension which the diaphragm, the intercostal muscles, and the rest of the muscles of respiration, had undergone from the pressure and weight of the confined matter, joined to the injury which the lungs themselves had sustained. He continued with very little alteration till the fourth day, when his respiration became worse, accompanied with bad nights, a quick pulse, a great discharge of foetid matter from the wound, and a flushing in his face. These symptoms remained till the 9th. To-day his breathing was better, his pulse became more calm, the flushings in his face disappeared; but the discharge was considerable in quantity, and he complained of a want of appetite, which, till now, had been tolerably good ever since the operation. On the tenth day, his difficulty of breathing returned; the discharge was very great, and offensive; he had very little sleep last night: in short, he seems greatly upon the decline. 11th, 12th, and 13th, he continued much as on the 10th. 14th and 15th days he has had more sleep than usual; his appetite very bad; cough frequent, and troublesome; discharge very thin, and considerable in quantity. Upon exerting his powers in the act of expiration, to discharge the matter from the cavity, there appeared

an eminence betwixt the 7th and 8th ribs (counting from above) on the anterior lateral part of the thorax, which was very tender; but, upon his ceasing to exert himself, the swelling disappeared. 16th and 17th days, his cough very troublesome; the discharge from the wound was great; the matter very thin, and foetid; he gets very little sleep; his pulse quick, and low; his tongue dry, and parched: in short, to all appearance, he seemed to have a very short time to live. But, notwithstanding these threatening symptoms, he very unexpectedly survived till the 10th of June following (exactly 12 weeks from the time of his undergoing the first operation). In the middle of the month of April next preceding his death, he thought himself much better than he had been for many days before: his discharge at this time was much lessened; but his pulse was very weak, and fluttering.

About three weeks before the patient's death, a second opening was made by incision betwixt the 11th and 12th ribs by my colleague Mr. Way, under whose immediate care this poor man was from the day of his admission into the hospital, and who treated him with the greatest humanity and judgment. Three days before the patient's decease, the swelling betwixt the 7th and 8th ribs burst of itself, and discharged at least a pint of matter, that was exceedingly offensive.

Upon opening the body, the following particulars were discovered: the right portion of the lungs was greatly ulcerated on its superior part, where it adhered firmly to the pleura: on the inferior part of the cavity, the lungs adhered to the diaphragm; the lobes

lobes of the lungs on this side were not so much wasted as I expected they would have been, from the great degree of pressure which they had so long sustained from the prodigious quantity of matter, that was confined in the opposite cavity of the thorax; nor was there any extravasation in the right cavity of the thorax.

The lobes of the lungs on the left side were almost entirely destroyed: in this cavity there was near a quart of foetid matter; the whole of its internal surface was ulcerated, and the two inferior ribs were carious in the neighbourhood of the second opening. In every other part the ribs were sound; so was the sternum.

The pericardium and heart appeared in their natural state. The injury done to the internal surface of the left cavity of the thorax was so great, as to have destroyed almost the whole of the intercostal muscles on that side of the trunk.

N. B. It may be worth remarking, that this patient did not any time suffer the least inconvenience from the pressure of the external air, which entered into the cavity of the thorax thro' the incisions, as has been said by authors to have happened in a great degree in the like cases; but as that effect was not produced in this, or any other instances of the like kind, which has come under my inspection, I am inclined to conjecture, that the bad effects of the pressure of the external air, when admitted thro' an opening made into the cavity of the thorax, is such an inconvenience as may rather be supposed to be likely to happen, than has been really known to happen often; the act of breathing having never been in

in the least suspended, altho' the openings made into the cavity have been very large, and the time taken for dressing the patient (sometimes once, sometimes twice a day) has been very considerable; but, on the contrary, at every dressing, I have observed, that the patient has breathed with greater freedom and ease than he could do before the performance of this operation, or even for some hours before the dressings were renewed. As the matter in this disease lies loosely in the cavity of the thorax, there is no greater danger of wounding the lungs in this operation, than there is of wounding the intestines or omentum, in tapping the abdomen in the ascites; and if the method be pursued, which I have recommended for making the incision thro' the intercostal muscles, there will be no danger of wounding the intercostal artery, which is a circumstance much dreaded by some practitioners, but with no great reason. However, was this to happen, it would probably not be attended with any bad consequences, as the bleeding might very readily be stopped by pressure, or otherwise.

Hatton Garden,
June 28, 1759.

Jos. Warner.

XXIII. *Extracts of some Letters from Signor Abbate de Venuti, F. R. S. to J. Nixon, A. M. and F. R. S. relating to several Antiquities lately discovered in Italy.*

L E T T E R I.

Rome, May 1st, 1757.

Dear Sir,

Read June 28.
1759.

— **I**N an inscription, which I found, while the front of the church of St. John Lateran was erecting, and which is now in the gardens of cardinal Corfini, without the Porta Aurelia (or S^{to} Pancrazio) mention is made of the * *equites singulares*, as guards of the persons of the emperors.

Herculi Invioto Sacrum
Genio Num. Eq. Sing.
Augg. N. N. Pro Salute
Imp. Cesar. L. Septimii
Severi, et M. Aurelii An
tonini - - - -
Et Juliae Aug. Matri
Castrorum. Aaug.

- - - - Do
mus divinae. Trib.
Occo. Valente, et Octavio

* This communication was occasioned by Dr. Ward's account of an inscription found at Malton (Philos. Transf. Vol. XLIX. Part I. p. 69.) an extract of which had been transmitted to Signor de Venuti by Mr. N.

Pifoni. et. Ti. Exerc. Fi.

Titiano. et Aurel. Lupo

C. Julius Secundus

† . rexit. Ere. suo. Deo. Do. D

Dedit. Idibus. Sept.

Severo III. et. Antonino Au

gg

NN. Cos.

As * Commodus was not ashamed to enter the Lists in the amphitheatre, as a gladiator (as appears by an inscription, which I have lately published) I do not question, but that he might have a further ambition to be ranked among the *equites singulares* also.

Without the gate of S^{co} Paolo, in the way to Ostia, about 8 miles from Rome, there has been discovered, within these few days, a magnificent sepulchre of very large dimensions, and of a round figure. In the middle of it was a sepulchral urn covered all over with sculpture in no inelegant taste. Among the rubbish, on the outside, they met with this inscription in large capitals,

‡ C. TUCCIUS. L. F. TRO. DVVCMVR.
ACTIA. UXOR.

Not far from this sepulchre, there are now found *ædes rusticæ*, peasants houses, adorned with rich marbles in the walls, and with statues.

† On the left-hand side.

* This refers to Dr. Ward's Account, p. 79.

‡ *Note.* To this family (perhaps) might belong M. Tuccius, who was *ædilis curulis*. Liv. l. xxxv. c. 41. Prætor. Ib. l. xxxvi. c. 45. & triumvir. Ib. l. xxxix. c. 23.

L E T T E R II.

Rome, Nov. 5. 1757.

DURING my summer recess at Viterbo, as I was tracing out the remains of antiquity in the adjacent country, I dropt, by mere accident, upon the ruins of Ferentum, a town of Etruria, different from that of the same name in Latium, near Mons Albanus. Here, besides the walls of the city, consisting of wrought square stone, I had the satisfaction of finding a temple built of the same materials, of neat workmanship, and a very elegant stile of architecture: but what surprised me more was a theatre almost perfect, not only in the circular part of it, but also in that, which was taken up by the scene or stage. It had its porticos intire on the outside, and likewise three entrances, answering to the *valvæ regię*, and the *hospitalia*, described by * Vitruvius: so that nothing was wanting to render it complete, but the *orchestra* and *pulpitum*. These remains are accessible to all the world; yet no one hitherto has delineated or published them. We have several valuable monuments in Latium, Sabina, Etruria, Campania,

* The learned abbate refers here (I presume) to Vitruv. de archit. l. v. c. 6. (Cunei) qui sunt in imo, et dirigunt scalaria, erunt numero septem; reliqui quinque scenę designabunt compositionem, et unus medius contra se *valvas regias* habere debet, et qui erunt dextrā ac sinistrā *hospitalia* designabunt compositionem.

Ipsę scenę suas habeant rationes explicatas, ita uti medię valvę ornatus habeant aulę regię, dextrā ac sinistrā hospitalia. Ib. c. 7.

For a fuller account of these entrances into the ancient theatres, vid. Montfauc. Antiq. explic. Tom. III. Par. II. l. ii. cap. 2, 3, & 4.

and Calabria, which contain subjects of the highest erudition, but yet yet are unknown to, and disregarded by, learned men ; while at the same time they are searching, with great expence and labour, after others in Greece and Asia, which are already known, and perhaps not so intire as these. I have caused a drawing to be made of the theatre above-mentioned, and some time or other (probably) may offer it to the public.

I have lately met with a curious dissertation, published by a professor of the university of Pisa, upon a gem, which exhibits the Theban war, with the names of five heroes engraved in Etruscan characters upon it. You (in all probability) saw it at Florence, in the cabinet of Baron Stofch, who a few days ago was struck with an apoplexy, and lies now at the point of death.

L E T T E R I I I .

Rome, Jan. 31, 1759.

A FEW months ago, I published a dissertation upon a little marble relievo, inscribed with Greek characters of the smallest size. The subject of it is the story of Circe, as related by Homer, *Odyss. lib. x.* It is really remarkable, that whereas there are extant several ancient monuments alluding to the *Iliad* of that poet, very few are found, which refer to the *Odyssy*.

There has lately been discovered without the *Porta Prænestina* (or Maggiore) about four miles from Rome, an old sepulchral apartment, wherein were
four

four sarcophagus's, adorned with very curious reliefs. Two of them were of a large size ; the other two of a smaller. On the first of the large ones was elegantly carved the Fight of the Giants, who are represented with thighs composed of serpents. The second appeared to me to express the Combat of the Amazons and Theseus. Of the two smaller sarcophagus's, one exhibited Bacchus in a chariot drawn by centaurs, and preceded by the whole chorus of the Bacchantes : the other seemed to be a battle (perhaps) between the Greeks and the Trojans ; as one part of the figures had long beards, tunics, and long breeches : all of them were of exquisite workmanship.

There has been lately dug up here an admirable statue of Venus, with an elegant Cupid standing upon a dolphin ; as also several curious inscriptions.

I have communicated your extract of Mr. Swinton's most learned conjectures concerning a coin of Monefes *, a Parthian king, to Signor Corsini. He approved of the performance, and admired the judgment and acute penetration of the author : but excepted a little to that part, where, in order to ascertain the epoch of the piece, he [Mr. Swinton] expresses a doubt with regard to the *Victory* on the reverse of it ; as there was no engagement that year between the Romans and the Parthians. But when Monefes had usurped the throne, dispossessed the lawful heirs, and plundered the neighbouring nations, it is no wonder, that he should stamp a *Victory* on his coin : which symbol ought to be referred to some

* Philos. Transact. Vol. L. Part I. p. 175.

considerable advantage gained over the Barbarians, and not over the Romans.

XXIV. *An Account of some Experiments relating to the Preservation of Seeds: In Two Letters to the Right Honourable the Earl of Macclesfield, President of the Royal Society. From John Ellis, Esq; F. R. S.*

My Lord,

London, Jan. 18. 1759.

Read Jan. 18,
1759.

AS the supplying our colonies with the seeds of useful plants, in order to have their produce imported from thence into England, instead of the places of their natural growth in Europe, Asia, and Africa, as we do at present, is a matter of some importance, therefore I am persuaded, that experiments tending to promote so useful and beneficial a work, will meet with the approbation of this honourable Society.

Among many useful seeds, which I sent governor Ellis in the year 1757, were some acorns of the cork-tree, which were put in a box in sand. These, he mentions in his last letters, were intirely spoiled in the voyage; and observes, that the confined air in the hold of ships, occasions such hot and penetrating steams, especially in warm climates, that it disposes all seeds, in common packages, to a sweating or putrefactive fermentation, by which the vegetative quality of many is intirely destroyed: and therefore advises,

vifes, that feeds ſhould be ſent in tight caſks, and placed on or near the deck, ſo as to have the benefit of the freſh circulating air, at the ſame time the tightneſs of the caſk would ſecure them from the ſalt water.

In order to ſend the governor a freſh ſupply of cork acorns, in a growing ſtate, I tried the following experiments on them to preſerve them ſound; the effect of which I expect to have the honour to lay before this Society next ſummer: but as I tried the very ſame experiments, at the very ſame time, on a parcel of freſh oak acorns, which I collected myſelf at Sydenham in Kent, the latter end of laſt October, and have ſince kept them by me in a box in a warm room, it may give us ſome inſight into what may be the fate of thoſe that are ſent abroad.

The experiments were made between the 25th and 30th of October 1758; and the acorns cut open to ſee the effects, Jan. 17, 1759.

Experiment 1. Acorns of the Engliſh oak ſmeared over ſeveral times with a ſtrong ſolution of gum arabic; and alſo they had been dried in a window, folded in a piece of paper, and put into a deal box.

When theſe were cut open, they appeared hard, dry, and inclining to black, being quite perished.

When I firſt thought of making this experiment, I imagined, that the perſpirable matter of the kernel of the acorns could not paſs through the glaſſy, cloſe ſubſtance of the gum arabic; but experience has convinced me of the contrary.

Exp. 2. Some acorns, treated as in the firſt experiment, were wrapped up in papers, ſoaked in a ſtrong ſolution of gum arabic, each in a ſeparate paper:

after they had been dried, they were put in the box with the rest.

These were somewhat softer than the first, but decayed.

Exp. 3. Some of them were smeared several times over with gum senega ; and when they were dried in the window, and well hardened, were put in a paper into the deal box.

These looked rather better than the two former parcels ; but unfit for vegetation.

Exp. 4. Some of the same acorns were put into the middle of a cake of plaisterers stiff loam, or such as the brewers use to stop their beer barrels, and covered over near an inch on every side. This soon became dry, without any cracks : it was about $2\frac{1}{2}$ inches thick ; and was placed with the rest, wrapped up in a paper, in the box.

The kernels of these were shrivelled up, and grown quite dry and hard, like horn, the loam proving a strong absorbent.

Exp. 5. Some were rolled up separately in thin flakes of bees-wax, warmed, to make it pliable, and put in paper in the box.

These looked very well when they were cut asunder, and appeared likely to grow ; but were a little shrunk.

Exp. 6. Some were rolled separately in rosin, made pliable with warmth.

These cut quite fresh.

Exp. 7. Some of them were rolled, each in a thin covering of a mixture of pitch, rosin, and bees-wax, called mummy by the gardeners.

These cut as well, and looked as fresh, as if they had just fallen from the tree.

The

The cork acorns, that were sent to Georgia, were inclosed in the same substances with the foregoing, and put into a box filled with dry sand, quite full, and well fastened: this was put into a tight cask, among papers and wearing apparel, and stowed in the upper part of the hold of the ship.

While I was making these experiments, I wrote to Dr. Linnæus, of Upsal, for his opinion of them, and for his method of preserving seeds in long voyages. I have lately received his answer; in which he considers the great danger that attends seeds in warm voyages, in the same light with governor Ellis, and has communicated to me a very probable method of preserving seeds in long voyages, which, he says, has never failed. The following is an extract of his letter to me, dated the 8th of Dec. 1758, from Upsal.

“ Seeds may be brought from abroad in a growing state, if we attend to the following method:”

“ Put your seeds into a cylindrical glass bottle, and fill up the interstices with dry sand, to prevent their lying too close together, and that they may perspire freely through the sand; then cork the bottle, or tie a bladder over the mouth of it. Prepare a glass vessel, so much larger than that which contains the seeds, that, when it is suspended in it, there may be a vacant space on all sides of about two inches distance between both glasses, for the following mixture; four parts of nitre, and one-fifth part, of equal parts, of common salt, and sal ammoniac: these must be well pounded, and mixed together, and the spaces all round between the outward and inward glasses well filled with it.”

“ This saline mass, which should be rather moist,
 “ will always be so cold, that the seeds in the inner
 “ glass will never suffer, during their voyage, from
 “ the heat of the air.

“ This experiment has been tried, and has not
 “ failed.”

I am, my Lord,

Your Lordship's

Most obedient, humble servant,

John Ellis.

My Lord,

Read Dec. 20, 1759. **I**N a letter, which I took the liberty to address to your Lordship, dated Jan. 18, 1759, relating to some experiments, which I had made to *preserve the acorns of English oaks* for a longer time than usual, in a perfect state of vegetation, I there took notice, that I had sent some acorns of the cork oak to the governor of Georgia, preserved in much the same manner: but as the substances I made use of for this purpose differed a little, I shall describe those experiments here more particularly.

On the 27th of Nov. 1758, I prepared seven parcels of the acorns of the cork-bearing oak or ilex, in the following manner:

N^o 1. 15 acorns, each covered over singly with a stiff solution of gum arabic, and afterwards rolled up in gum'd paper.

N^o 2. 13 D^o. each rolled up in a thin cover of common yellow bees-wax, softened before the fire, and rolled up afterwards, separately, in white paper.

N^o 3.

- N^o 3. 10 D^o. *each* rolled up, as before, in *wax*, and afterwards each covered with a coat of *brewers loam moistened* with a thick solution of *gum arabic*.
- N^o 4. 5 D^o. *each* coated with *gum arabic*, and afterwards with *whiting moistened* with a thick solution of *gum arabic*.
- N^o 5. 25 D^o. *each* coated with *gum arabic*, and afterwards with *brewers loam moistened* with a thick solution of *gum arabic*.
- N^o 6. 3 D^o. *each* covered with *gardeners grafting mummy*, consisting of a mixture of *bees-wax*, *rosin*, and *pitch*.
- N^o 7. 10 D^o. *each* covered with *fullers earth* made into a *paste* with a stiff solution of *gum arabic*.

These seven parcels were all put into chip boxes, filled with *dry house-sand*, and afterwards put into a *tight cask*; and arrived in Georgia in *April* following. Governor *Ellis*, in his *letter* to me, dated from thence *May 6, 1759*. says, of all these experiments, *none succeeded but the parcel N^o 3.* which had first been covered with *bees-wax*, and afterwards with a paste made of *loam and dissolved gum arabic*. We even find, that those that were covered with a thin coat of *bees-wax*, and afterwards with *paper*, did not succeed; as their covering was not thick enough to keep in their perspiration. This was the case with some of the *English oak acorns*, which I had coated in the same manner in *October 1758*, and cut open in *November last 1759*; their kernels being *shrivell'd and decayed*: and those I had covered the same time with a mixture of *rosin, bees-wax, and pitch*, tho' their

kernels were *plump* and juicy, yet they, by this time, were *turned brown and rancid*, by imbibing the *steams* arising from the pitch and rosin, and were rendered *unfit for vegetation*.

It may possibly be remarked, that it is no uncommon thing to receive the *acorns* of oaks *from* most of the provinces of *North America* in a growing state in *January*, and even in *February*; and therefore it may be asked, *why* it should require *more care* to send acorns of our growth *thither*?

The reason of this appears to me, that as the summer heats of those provinces by much exceed ours; so consequently *their juices being higher maturated*, are not so liable to shrivel and decay as *ours* are; which, experience shews, are more *watery*, and *less oily*: tho', perhaps, if both kinds were packed up in a *dry, soapy earth*, and could be carried at a *cool season* of the year, I mean the winter months, they *might equally succeed*; but, in this kind of weather, we have *seldom an opportunity* to send them, so as to expect their arrival before the weather, in the southern parts of *North America*, begins to grow too warm, as the ships seldom arrive there till April.

The chesnut, next to the acorn, being the most difficult to preserve sound during the course of one season, or a whole year, on the 23^d of *February* last, 1759, I procured a parcel of *Spanish chesnuts*, just as they were imported, many of which were sounder than they generally are so late in the season: these I divided into *four parcels*, and put *each* parcel into a small *earthen jar*, involving them in the following substances:

- Jar N^o 1. 12 Chesnuts in mutton suet.
 2. 12 D^o. — in bees-wax and mutton suet,
 equal quantities.
 3. 12 D^o. — in bees-wax.
 4. 12 D^o. — in bees-wax and yellow rosin,
 equal quantities.

These substances I *melted*; but did not pour them among the chesnuts, till I could *bear* my *finger* in them without the least sensible uneasiness, which I considered as the proper test not to affect the kernels by their heat, and immediately *immersed the jar* to the brim in *cold water*.

As this experiment was made with a *view* to give those gentlemen some hints, who go to the *East Indies*, I placed these jars in a room, where they were *exposed* to the *unusual heats* of last summer; heat being the great promoter of the putrefactive fermentation of vegetables, and which it is very hard for such gentlemen to guard against, especially as they are obliged *twice* in their voyage home to *pass the equinoctial line*.

In order to examine the effects of these experiments, and to lay before the Society a fair account of them, I *broke* all the *jars* on the 22d of November last, before some ingenious gentlemen of the Society, very intelligent in these matters, and found, that jar N^o 1. which contained the chesnuts immersed in mutton suet, proved *all rotten*, attended with a very disagreeable *putrid smell*. Those in jar N^o 2. were *most* of them *sound and fresh*, and their kernels as white and sweet-tasted, as when *fresh gathered*. These were inclosed in half bees-wax, and half mutton suet, melted together. Those in jar N^o 3. were
equally

equally sound and well tasted, and had been inclosed in bees-wax only.

Though *part* of the chefnuts in these jars were rotten; yet it appeared plainly to be *owing to some defect* in them when they were *first* immersed into these substances; most probably to the lateness of the season, when the experiments were made.

Those in jar N^o 4. which were inclosed in half bees-wax and half yellow rosin, were *all turned soft and spongy*, of a *brown* colour, and a most *disagreeable taste and smell*, from the refinous steams they had imbibed.

On the 24th of November last, I *planted six* of the chefnuts preserved in wax and suet (N^o 2.) and *six* of those preserved in wax only (N^o 3.) in two *garden pots*, and placed them in a very spacious *conservatory*, belonging to my worthy friend Philip Carteret Webb, Esq; F. R. S. at his seat near Godalmin in Surry; where I have the pleasure to inform your Lordship and this honourable Society, that *many of them are already germinating*; which *proves* this *method* of preserving the larger seeds a very *proper* one to recommend to gentlemen that go to *China*, and other parts of the *East Indies*, to preserve many kinds of valuable seeds in a state of vegetation during a voyage of a *whole year*, till they arrive here; and probably till they are carried to our settlements in the American colonies.

It remains then, for gentlemen who go to the *East Indies*, to place the seeds they preserve in *bees-wax*, or *bees-wax and suet*, in the *coolest* part of the *ship*, to prevent these substances being affected with the heat of those parts, which far exceeds ours. *Perhaps* Dr. Linnæus's method of inclosing them in a larger vessel,

vessel, and surrounding them with a *mixture of salts*, described in my former letter, will *answer this end*. He speaks with so much certainty of its success, that I think it worth the trial, especially when he *assures* us it *never fails*.

I am, my Lord,

Your Lordship's

most obedient humble servant,

London, Dec. 13. 1759.

John Ellis.

P. S. Small seeds, in their pods, may be preserved by being placed thinly on pieces of paper, cotton or linen cloth, that have been dipt in wax, then rolled up tight, and well secured from air by a further covering of wax.

XXV. *The Case of a very long Suppression of Urine.* By Ambrose Dawson, *M. D.*
Communicated by William Heberden,
M. D. F. R. S.

Read Nov. 8, 1759. **R.** W. aged 23 years, tall and well-made, was seized, in the year 1755, with a weakness of one side, which soon went off, leaving only one knee weak and swelled; for which she was admitted into St. George's Hospital.

On the 4th of April, 1756, she had a stoppage of urine, and felt no disposition to make any for two days. During the whole month of April, the discharge of urine was very irregular, it having ceased at one time for five days, and at another time for nine days. When the catheter was introduced, little or no urine was found in the bladder.

In order to relieve her, she had been directed to use both the warm and cold bath, bleeding, purging, turpentine clysters, and a strong infusion of pareira brava, without any success: cantharides had likewise been given her to the quantity of three grains in a dose; and this dose had been repeated four times in 24 hours; but without any other effect than that of occasioning pains in her throat, stomach, and bowels, with vomiting and purging. A wet towel put round her waist, seemed to do the most good, and brought away some water two or three times; but afterwards ceased to have this effect.

From the 26th of April, 1756, she made no water, and felt no want of making it, for many months; yet all this time she could eat once or twice a day, and was able to walk and ride. She was sparing in the use of liquids; she had but little sleep, no sweats, and her skin had no urinous smell: her breathing was often very laborious, with a dry cough: the catamenia were irregular: there were oedematous swellings in her limbs, abdomen, hips, and face; but, by the help of purges, and spontaneous vomitings, which began in the third month of the suppression, these swellings were tolerably kept under. She vomited sometimes every day, and sometimes only every third or fourth day; and tho' these vomitings usually came on presently after dinner, yet what she vomited seemed to be mere urine, without any thing which she had eaten mixed with it. In the beginning of June 1757, the nipples of her breasts chopped, and discharged sometimes a watery humour, sometimes a thick matter, streaked with blood, and sometimes a humour approaching to the colour of urine: all these dif-

discharges had an urinous scent, and seemed to lessen her urinous vomitings, which, from this time, became less frequent: but her legs, and especially her body, swelled to an extraordinary size, and she breathed with the utmost difficulty.

On the first and second of August, 1757, after a total suppression of urine for above a year and three months, she felt uncommon pricking pains, with great heat all down her back and loins, and about the belly and groin. On the second day, she voided about three ounces of thick, slimy matter, attended with sharp pains in the urinary passages: this water was not high-coloured. The next day, she made water of a truly urinous kind with less pain; and continued to make a little water every day, the pain daily decreasing; and on the 7th she voided about a pint.

Since this, she has often had a suppression of urine for ten or fourteen days; and it once lasted two months, during which she had no vomiting; but her body was very much swelled. In July 1759, according to her own account, she did not usually make above half a pint in 24 hours; and sometimes scarce so much in two days. The catamenia were then irregular; her sleep short, and disturbed; she had very little appetite; her legs swelled; and the rest of her body wasted.

XXVI. *Several Accounts of the fiery Meteor, which appeared on Sunday the 26th of November, 1758, between Eight and Nine at Night; collected by John Pringle, M. D. F. R. S.*

Read Feb. 8, 1759. **S**INCE the paper concerning the meteor, of November 1758, was read at the Society, I have been favoured with several more observations relating to the same appearance, which I have connected with the former in such a manner, as that we may now trace the course of that body, from the south northwards, over a considerable part of this island; and at the same time form a better judgment of its figure, height, magnitude, and velocity, than what could be drawn from the first accounts only. At present, I shall lay before the Society all the inquiries I have made on this subject, and, in another paper, I shall offer a few remarks, containing the result of these observations.

I. I said at first, that I had not heard, that this body was observed any-where south of London, except at Silchester; but having since read in one of the Magazines, that it was seen at Plymouth, I wrote to Mr. Mudge, Surgeon in that place, to be informed about the truth of that article; who answered, “That
 “ he had made every inquiry in his power with re-
 “ gard to the meteor, and could find but two per-
 “ sons who pretended to have seen it, and those to-
 “ tally disqualified from giving him any satisfactory
 “ account

“ account, either as to its magnitude, direction, the
 “ angle with the horizon, or degree of light: that
 “ they only saw in general (after the body of the
 “ meteor had passed) a red glaring flash, which
 “ seemed rather to excite astonishment in them than
 “ curiosity. In short, that nothing more could be
 “ collected, than that it was, or might have been,
 “ seen at Plymouth.”

Some time after, Mr. Mudge favoured me with another letter; in which he says, “ I think I can
 “ now venture positively to say the meteor was not
 “ seen at Plymouth. Besides a very minute and particular scrutiny among the people of the town, as
 “ I was apprehensive the narrowness of the streets,
 “ and height of the houses, might have been the
 “ cause of their not observing it, the Lieutenant-governor was so obliging, at my request, to send a
 “ serjeant to inquire of every foldier in the garrison;
 “ and as some of them must have been on sentinel
 “ duty that evening, I am thoroughly persuaded, if
 “ the meteor had appeared above their horizon, it
 “ could not have escaped them, as the garrison is
 “ situated on an eminence, and the prospect bounded
 “ by the sky only.”

Dr. Huxham also acquainted me, that he did not believe the meteor had been seen at Plymouth.

II. The reverend Dr. Shipley, minister of Silchester in Hampshire, a parish about 45 miles * W. S. W.

* Unless where it is otherwise expressed, I would be understood to use all along the standard English measure of $69\frac{1}{2}$ miles to a degree.

of London), told me, " That he had not a view of
 " the meteor himself, but had conversed with three
 " countrymen, his parishioners, who had seen it :
 " that they had all agreed in observing the light to
 " be greater than that of moonshine ; and that one
 " of them, in particular, said, it was so great that he
 " could easily have seen a pin lying on the ground :
 " that the body at first was like a large shooting star,
 " but with a slower motion : that its direction was
 " northerly : that, during its progress, it increased in
 " size, leaving a stream of light behind ; and at last,
 " as it declined to the horizon, its lower part became,
 " in appearance, as broad as his hand, whilst the
 " length of the whole seemed to be about five feet,
 " of a conical figure, ending in a point upwards :
 " that, before it reached the horizon, it burst into a
 " flame, resembling a flash of lightning, and then
 " immediately disappeared *." Dr. Shipley going to
 the spot where the observer had stood, and making
 him point to some trees at a distance, over which, he
 said, the meteor disappeared, the Doctor found, by
 taking the altitude with an instrument, that it had

* From this observation it appears, that the stream of light, called the tail, was not seen at first ; probably because the meteor was in too high a region for the air to make any resistance to the flame ; but when the body descended lower, then the air, tho' still extremely rarified, yet, from the extraordinary velocity of the meteor, would make some opposition to it, and drive the flame backwards to form the tail. That the meteor descended obliquely, will be more fully shewn afterwards, tho' the circumstance mentioned above of a conical figure ending in a point upwards, as it declined towards the horizon, is one proof of that fact.

been extinguished about $1^{\circ} 15'$ above the horizon, then bearing 35° westward of the north †.

III. As for this city, and the parts adjacent, all I could learn was likewise from a Magazine; in which it was said, "That the meteor was seen by three gentlemen in Chelsea-fields." It is probable, that, on that evening, the air was foggy hereabouts, or that there was no wind to carry off the smoak; for these circumstances will easily enough account for there being no notice at all taken of that body in London, and that it was so little heard of in the neighbourhood.

IV. Having heard it was seen at Colchester in Essex, I desired the favour of Mr. Windham Bowyer, commissioner of the excise, to employ some of the officers of that district to procure what intelligence they could about its appearance there. Accordingly Mr. Wigson, collector at Colchester, informed Mr. Bowyer, in answer to the queries sent him, "That he had found a person who had seen the meteor, on the 26th of November, about eight in the evening; and who said, that its direction, to the

† This bearing carries the meteor about a point farther to the westward than what is consistent with the common maps, and several of the following observations.

Upon the supposition that the observer was tolerably exact in pointing out the apparent altitude, at which the meteor disappeared, and that it was extinguished when nearly perpendicular to Fort William (in the Highlands of Scotland), as shall be shown afterwards, then, allowing 22 miles for the curvature of the earth, and a distance of 420 miles between Silchester and Fort William, the real height of this body, at its disappearance, was about 32 miles.

" best

“ best of his judgment, was about south-east ; that
 “ the apparent diameter of the body was about five
 “ or six inches, not so large as the moon when at
 “ at the highest, but more bright ; that the meteor
 “ left a train of light behind it ; that its progress was
 “ extremely swift ; that no explosion was heard
 “ when it disappeared ; and that he did not perceive
 “ it to break into stars in the manner of a rocket.”

Finding this account disagreed so much with those I had received from other parts, with regard to the course, I wrote to Mr. Wigson, begging he would once more see the person, and desire him to point out the path of the body, in order that I might be satisfied he had not been mistaken about its motion to the south-east ; and, in return to my letter, that gentleman acquainted me, “ That he had
 “ again conversed with the observer, who still persisted in describing the course of the ball from the
 “ north-west to the south-east ; adding, that it appeared, at its greatest height, to have the same altitude which the sun then had (March 12), at 10 in
 “ the morning * ; but that it inclined to the horizon
 “ with great rapidity, and disappeared intirely, without dispersing, seeming to him to fall into a wood.” Mr. Wigson concluded with observing, “ that as this
 “ man was at that time on a journey from Thorp
 “ to Colchester, he might easily be deceived as to the
 “ points of the compass, by the windings of the
 “ road.”

V. In tracing the progress of this body northwards, I was favoured with the following letter from

* Viz. about 32°.

Fig. I.



Fig. II.

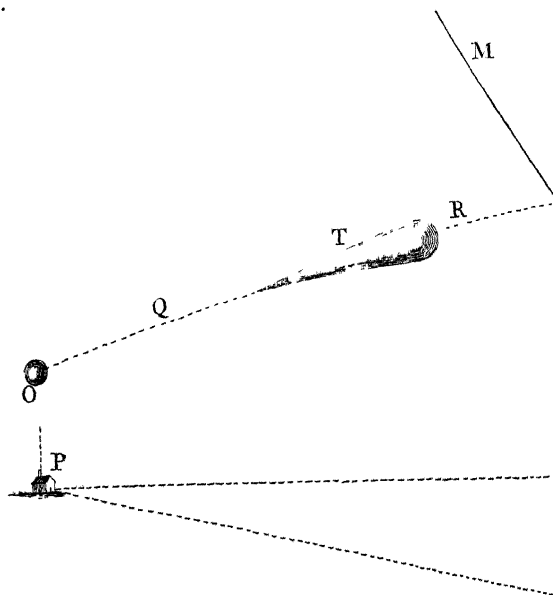


Fig. V.



the reverend Mr. John Michell, fellow of Queen's College, Cambridge. " I promised to send you what
 " further account I could get from the person here,
 " a glazier by trade, who saw the meteor of the
 " 26th of November. I walked yesterday along
 " with him to the place where he was when he saw
 " it, taking with me a quadrant, to measure such
 " altitudes as he was able to give me from his me-
 " mory, and a compass to take the bearing. The
 " first appearance, by the account he gave me, was
 " at least 70° high, and it appeared to move directly
 " perpendicularly *, till it came down to the horizon,
 " where it passed between two trees, which he point-
 " ed out to me : this last place of its appearance was
 " 23° west of the north from the place where he
 " stood ; and, as we were at least a mile distant
 " from the trees, I believe you may depend upon that
 " bearing to a degree or two at most. The whole
 " time of the appearance was (as near as my informer
 " can guess), as long as he should be in walking near
 " 400 yards ; but in this, I imagine, he is somewhat
 " deceived, as I think I can collect, both from his
 " own account of it, and that of another person who
 " was with him, the time was much shorter. The
 " head, which went foremost (*Plate VII. Fig. 1.*)
 " was, by the description, of a bright white, like
 " iron, when almost of a melting heat ; but it emit-

* In a second letter, Mr. Michell says. " He asked the observer
 " several times, whether the direction of the meteor did not vary
 " to the right or left ; and that he had assured him, that, accord-
 " ing to the best of his judgment, it appeared to move exactly per-
 " pendicularly ; whence it must have crossed the meridian in the
 " zenith, and moved in a great circle with regard to Cambridge."
 " ted

“ ted no sparks, as iron does in that state. The head
 “ was about half the diameter of the moon, and, till
 “ it had descended to within about 14° of the hori-
 “ zon, was, as I apprehend by the account, some-
 “ what less in the vertical than in the horizontal dia-
 “ meter; but, from 14° high, it was at its utmost
 “ splendor, and round, and continued so till it disap-
 “ peared. The tail was about a fifth part of the
 “ breadth of the head (*Fig 1.*), and when the head
 “ was about 27° high, was at the longest: the length
 “ then might be somewhat more than 8° , which
 “ was the mean length. The colour of the tail was
 “ a duskyish red, about the colour of red-hot iron,
 “ all of a breadth, not pointed. When the head was
 “ about 6° or 7° high *, the tail burst, as my
 “ informer expressed it, and the brightness of the
 “ light dazzled his eyes; after which the tail disap-
 “ peared, and in the room of it there were three
 “ stars, all contained within the compass of a little
 “ more than one degree from the head (*Fig. 2.*); and
 “ they, together with the head, descended, keeping
 “ their due distance, till below the horizon †.
 “ The diameter of these stars was nearly the same
 “ with the diameter of the tail, viz. about $3'$; but they
 “ were of the same colour as the head. The
 “ brightness of the light was so great, that (accord-

* Supposing this angle of elevation just, the real height of the meteor, when the tail broke off, over the shire of Lanerk in Scotland, was about 42 miles, allowing for the curvature of the earth.

† This circumstance of the head's falling under the horizon might have been a deception; for at Silchester, which was at a greater distance, the head disappeared before it came to the horizon; but, perhaps, the horizon was clearer and lower there than at Cambridge.

“ ing to the account I received) one might see to
 “ pick up a pin, and some noise was heard; but of
 “ this latter, at least, I a little doubted, imagination
 “ being able to help out a good deal in such cases.
 “ The appearance was about half an hour after eight
 “ in the evening, on a Sunday; but the day of the
 “ month, or the month itself, the man does not
 “ certainly remember. This is all the account I have
 “ been able to procure. The heights, &c. I could at
 “ first only get in, *as long as from here to that hedge;*
 “ *as broad as a hat crown; as thick as my wrist; and*
 “ *about as long as a broom-staff:* but, as I thought these
 “ would give you no more ideas than they gave me, I
 “ desired my man to point out in the heavens, as near
 “ as he could guess, clouds, &c. of the same size,
 “ and then I measured them with a quadrant, and
 “ the result of these measures is what I have sent
 “ you.”

VI. Pursuing the progress of the meteor north-
 wards, my next information was from Manchester.
 Mr. Lloyd, of that place, Fellow of the Royal So-
 ciety, wrote, that tho' it had been seen there by se-
 veral, the only tolerable account he could send me
 from that country, was from Lord Derby's head gar-
 dener; who said, “ As he was returning from Liver-
 “ pool to Knowsley (a place at 7 or 8 miles distance),
 “ about eight in the evening, he was surprised by a
 “ sudden glare of light; and that he soon saw a ball
 “ of fire appearing, of half the breadth of the moon,
 “ moving horizontally eastward *, a little inclined to

* This person, like the traveller between Thorp and Colchester,
 has probably been deceived in the direction by the windings of the
 road.

“ the north, with a hissing noise : that a train of light,
 “ like a tail, followed it, which being soon collected
 “ into the body, it burst, and part seemed to fall down
 “ like stars, and the rest vanished. He thought the
 “ whole appearance continued about two minutes.”

VII. Mr. Lloyd added what follows from the Liverpool News-paper, dated 1st of December, 1758.
 “ On Sunday last was seen in West Derby *, by several credible persons, between 9 and 10 o'clock
 “ at night, a ball of fire, which arose in the east, and
 “ appeared to increase in size for some time, and then
 “ burst, without noise. Its direction was to the
 “ northward.”

VIII. Cockermouth, in Cumberland, is about 86 miles north by west of Liverpool. Mr. Muncafter, of that town, says, in a letter to a friend in London,
 “ That the meteor passed over that place † about
 “ nine in the evening, on November the 26th, with
 “ a very great velocity, towards the north-west ; that
 “ it gave so strong a light, that the smallest thing
 “ might have been seen on the pavement ; and that it
 “ disappeared in less than a minute : that the globe of
 “ fire appeared as large as the moon when she is high,
 “ but much brighter ; and had a tail of a conical
 “ form ; but that they did not observe any sparks or

* The district so called of Liverpool.

† *Over that place.* This expression we are not to understand here in the strictest sense, more than when used by the observer at Carlisle : besides, as the meteor really passed within a few miles of the vertical points of both those places, and probably was at that time above 60 miles high, it could not appear to decline much from the zenith.

“ stars fall from it, like those which are seen on the
 “ bursting of a rocket ; nor did they hear any ex-
 “ plosion.’

IX. From Carlisle, which lies about 26 miles N. E. of Cockermouth, the account I received was more particular, and for which I was obliged to Mr. James Hewit, wine-merchant of that city, who not only had a view of the meteor during part of its course, and heard a report, but, at my request, some time after, took the trouble to measure the height, at its apparent elevation when he saw it, and at its extinction, from the memory of another person, who had a sight of it to the last. I shall give the substance of several of his letters on this subject, in his own words.

“ The direction of the meteor was from south-east
 “ to north-west. It did not appear in a globular
 “ form, as it passed over Carlisle ; but tapered in the
 “ manner represented in *Fig. 3*. Its head seemed
 “ to be about 14 inches in diameter, and its length,
 “ from head to tail, about 5 yards. It appeared much
 “ brighter than the moon, and lightened the atmo-
 “ sphere to such a degree, that a person, who stood
 “ in the street, could easily have distinguished the
 “ difference between a small needle and a pin, if
 “ they had been lying on the ground before him. It
 “ emitted several sparks as it went along, and con-
 “ tinued in sight about 25 seconds. About a minute
 “ after it disappeared, there were two explosions im-
 “ mediately following one another, of a hollow
 “ noise, as loud as the report of a cannon at 2 or 3
 “ miles distance ; and, immediately after the explo-
 “ sions, there was heard a confused rumbling noise

“ in the air, which continued at least 20 seconds; at
 “ the same time I could perceive no wind, and the
 “ stars were remarkably bright. As for the greatest
 “ altitude, which you desire to be informed about, as
 “ I could very well retain that in my head, a gentle-
 “ man in town, and myself, took that height with a
 “ theodolite, by pointing the instrument to that part
 “ of the heavens I saw the meteor in; and in this I
 “ could be very exact, as I took particular notice of
 “ its apparent elevation above a certain high house
 “ it seemed to pass over. We found the greatest
 “ height to be 32 degrees above the horizon, on a
 “ vertical circle; and 41 degrees from the north to-
 “ wards the west *. As I did not myself see the me-
 “ teor extinguished, by reason of some houses that
 “ were in the way, I found a person in this neigh-
 “ bourhood, who had seen it to the last; and getting
 “ him to point to that part of the heavens where it
 “ disappeared, we also took that height with the
 “ theodolite, and found it eight degrees †. This
 “ man seeming to be pretty sure of his pointing to the
 “ very place in question, I believe you may depend
 “ on the observation as tolerably just. As for the
 “ figure (*Fig. 3.*), you have it as correct as I could

* It appears by this observation, that the meteor being so low
 as 32°, it must have passed the town a great way before Mr. Hewit
 got a sight of it.

† The meteor being extinguished when perpendicular to Fort
 William (as will appear by a subsequent observation), at the appa-
 rent altitude of 8° at Carlisle, makes the real height at Fort Wil-
 liam to have been between 26 and 27 miles, allowance being made
 for the curvature of the earth, of 3½ miles, between these two
 places.

“ make it ; only, perhaps, I have added too many
 “ sparks, as I doubt there did not so many issue from
 “ the tail. The meteor did not discharge any sparks
 “ nearer the thick end, than are described in the
 “ figure ; but those it did emit, darted from it with
 “ an astonishing velocity. In answer to what you
 “ require in your last (viz. whether the path of the
 “ meteor was to the eastward or westward of Car-
 “ lisle ?) my situation, when I saw it, was near the
 “ center of the town ; and the bearing of that part of
 “ the house, over which I saw it, was 41° from the
 “ north towards the west ; and as its progress ap-
 “ peared on the left side of me when facing the said
 “ house, the path was consequently west of me, and
 “ of any part of this city *.”

X. Mr. Jonathan Ormiston, merchant at New-
 castle, favoured me with all the information he could
 procure in that part of the country. It consisted of
 an abstract from the Newcastle Journal, and the copy
 of a letter from Mr. Martin Doubleday, one of his
 friends near Durham, whom he calls a gentleman of
 sense and knowledge. The article from the Journal
 is as follows. “ Newcastle, 26th of November,
 “ 1758. This night a surprising large meteor was
 “ seen here, just about nine o’clock, which passed a
 “ little westward of the town, directly to the north,
 “ and illuminated the atmosphere to that degree for
 “ near a minute, that, tho’ it was dark before, one

* From this last circumstance, compared with observations V.
 and XIII. we are enabled to judge nearly of the true path, which
 must have run from Cambridge across the Solway frith, between
 Carlisle and Dumfries, and by Obs. XVI. on to Fort William.

“ might have taken up a pin in the street. Its velocity was almost inconceivably great ; and it seemed near the size of a man’s head. It had a tail about two or three yards long ; and, as it passed, some say they saw sparks of fire fall from it. It appeared low in the atmosphere ; and we are advised from Edinburgh, that it passed over that city * just about the same time, had the same appearance, and moved in the like direction.”

Mr. Doubleday’s letter.

XI. “ On the 26th of November last, about a quarter before nine, as I was sitting writing by candle-light, with my face towards a window fronting the north-west, I was surprised by a sudden and extraordinary light, and stepping hastily to the window, saw the resemblance of a large sky-rocket, falling and bursting into sparks of fire, which became more scattered in its descent, and seemed to be quite spent by that time it reached the horizon (which it did, as near as I could guess, due north †), its path appearing luminous to a considerable distance from the scattered parts, which with it were not dispersed, but as if confined between two parallel straight lines. The greatest height of its luminous path, when I first saw it, was 25 degrees above the horizon, N. W. by N. I heard no noise at the time, nor after ;

* By the accounts I had from Edinburgh, it was not nearly vertical there.

† The head of the meteor, seen from this gentleman’s house, could not reach an unobstructed horizon, nor be seen due north by 3 or 4 points, consistently with most of the other observations.

“ and I conclude from its appearance, that it must
“ have begun to burst before I saw it.”

Dated from Butterby, about a mile
south of Durham *.

XII. Mr. Blake, Fellow of the Royal Society, sent me the observation of the reverend Mr. William Henderson, vicar of Felton (a village about 24 miles N. N. W. of Newcastle); who says, “ That
“ the night was dark and calm; that as he was going home (a little after nine), about 20 yards west
“ of the bridge, the road was instantly so much enlightened, that he might have seen to take up a pin;
“ that the globe, to the eye, was about the size of a
“ ball of 6 or 7 pounds weight †; that he could not
“ guess at its distance from the earth, but, during
“ the short time he saw it, he imagined he heard it
“ whizz over his head ‡; that it had a tail like that
“ of a comet, almost a yard in length, perpetually
“ emitting sparks of fire; that the time of observation
“ was very short, on account of a great hill that rises
“ on the south side of the river Coquet, and of a
“ clump of trees on the north side, which obstructed
“ his view; that its velocity was great, for that it
“ did not continue in sight above 5 or 6 seconds; and
“ that its course, as near as he could judge, was to
“ the north-west.”

* Durham is about 59 miles from Carlisle eastward.

† The observer probably means a cannon ball of a six-pounder.

‡ This found must have been a deception, as shall be shewn afterwards.

These were all the accounts of the progress of the meteor, which I could procure in England: I shall next lay before the Society the observations I have had from Scotland; and as I have begun, I shall continue to trace the appearance from the south northwards.

XIII. Dr. Gilchrist, physician at Dumfries (which lies a few miles north of the Solway frith, and about 30 miles N. W. by W. of Carlisle), acquainted me, " That the best account he could get of the meteor
 " (which he did not see himself), was from a young
 " man of that place, who, in common affairs, was
 " sensible and distinct. This person, on the 26th of
 " November, 1758, in the evening, being in a room on
 " a first floor, which had the windows to the north-
 " east, was surprised by an extraordinary light, and,
 " running to one of the windows, saw a large fiery
 " body, like red-hot iron. It appeared to him as large,
 " and as long, as a middle-sized man, the fore part
 " broadest; its progress was from S. E. to N. W. part
 " of the tail separated from the rest, but he still
 " thought it followed the body for a little space, and
 " then it burst like gunpowder, tho' without noise, and
 " fell down in sparks of fire, whilst the body kept on
 " its course*; but which he immediately lost sight
 " of, by a house of two stories high that intervened." The doctor said further, that the same person formed his judgment of its height " from one of the steeples
 " of the town, a hundred feet high;" but, as the

* The circumstances contained in this paragraph agree perfectly with the account of the farmer at Ancram. See Obs. XVI.

distance of this steeple from the observer is not mentioned, nothing can be made of the apparent altitude from that description, more than of the distance eastward, “ which, letting fall a perpendicular from the meteor, he imagined, was not above a good gunshot from him.”

Dr. Gilchrist added, “ That a young lady of his acquaintance, happening to be in the street near the same place, saw the meteor likewise, and described it as a ball of fire, about the bigness of the sun, with a tail; and the length of the whole as longer than one’s arm. She said, it was almost over her head *, higher than the steeple mentioned before; that it burst without noise, and was intirely dissipated into sparks of fire, which fell down, and, as she thought, almost reached the tops of the houses.”

XIV. The reverend Mr. William Turnbull, minister of Abbotrule (a parish about 46 miles N. by E. of Carlisle, 44 miles N. E. by E. of Dumfries, and 6 miles S. W. by W. of Jedburgh, in the shire of Roxburgh), favoured me with a letter, containing the following particulars, “ That on Sunday the 26th of November, 1758, about nine at night, sitting in his parlour, which had a south-west window, he very distinctly saw a light, which he took for a flash of lightning; but was surprised with the difference of its colour, as being whiter, and giving a clearer

* From this circumstance, and the greater apparent magnitude at Dumfries than elsewhere, it is probable the meteor was more nearly vertical there than at Carlisle, or any other place of observation mentioned.

“ view of the pictures, and every thing else in the
 “ room, than what he could have expected from com-
 “ mon lightning; that, however, he waited for a
 “ clap of thunder, and accordingly, at the end of five
 “ or six minutes, he heard a very great explosion, not
 “ indeed so like thunder, as the crashing noise of the
 “ fall of a house; and being persuaded that this was
 “ really the case, and that the gable-end of his own
 “ house, farthest from the room he sat in, with the
 “ offices, had fallen, he ran out, but found no damage
 “ done, nor saw any clouds, it being clear star-light.”

XV. Having written to Mr. Walter Pringle, she-
 riff-depute of the shire of Roxburgh, for what infor-
 mation he could give from that part of the country,
 he acquainted me, “That he himself had neither seen
 “ the meteor, nor heard the explosion; but that a
 “ servant of the house, where he happened to be
 “ that night (about 20 miles S. S. E. of Edinburgh,
 “ and as far N. N. W. of Abbotrule), came in
 “ about nine, and told him there had been some
 “ thunder and lightning; which he thought very im-
 “ probable, as he had been out but a few minutes
 “ before, and had not seen a cloud in the sky’.

Mr. Pringle added, that the reverend Mr. John
 Smith, of Jedburgh, had written to him as follows:
 “ I am surpris'd, that, in all the accounts given of
 “ the meteor, seen on the twenty-sixth of Novem-
 “ ber, one remarkable circumstance is omitted,
 “ namely, the horrid crack, which I heard, being
 “ then on the confines of Cumberland, near Stone-
 “ garthside (about 15 miles N. by E. of Carlisle): it
 “ was much louder than the report of any heavy
 “ cannon, and continued about 7 or 8 seconds. The
 “ people .

“ people thought it a peal of thunder ; but this, I
 “ imagined, could not be the case, as the sky was not
 “ clouded. I did not see the meteor myself, being
 “ then within doors.”

XVI. In another letter, Mr. Pringle informed me, “ That he had conversed with one James Turnbull, a farmer at Ancram, a village about three miles north-west of Jedburgh, who had seen the meteor, and heard the explosion, and who being a very sensible man, he thought he could not give me greater satisfaction, in answer to my queries, than by desiring him to write me a full account of what he had seen and heard.” Accordingly I received the following letter from that person.

“ At Mr. Pringle’s desire, I send you an account
 “ of the meteor, as it appeared to me on Sunday the
 “ 26th of November, 1758. My dwelling-house,
 “ at the mill of Ancram, fronts south-east ; and the
 “ mill-house, which is directly before it, has the same
 “ exposition, at the distance of about twelve yards.
 “ About nine at night I happened to be out, and
 “ upon returning to my house, and just entering the
 “ threshold, the whole side of the house became
 “ suddenly enlightened, and with a brightness as
 “ of sun-shine. My back being towards the place
 “ from whence the light came, I quickly turned
 “ about to see what might be the cause of it, and
 “ then beheld a globe of fire about the bigness of the
 “ crown of the cap I commonly wear, 6 or 7, or at
 “ most 8 inches in diameter *, directing its course
 H h 2 “ from

* After receiving this letter, I wrote to Mr. Smith, to desire him to ask Mr. Turnbull, how many inches in diameter the full moon appeared

“ from a shepherd’s house (which stood at above a
 “ mile’s distance) towards me, as I thought, and right
 “ over the middle of the mill-house. I scarce had time
 “ to think, when it passed by me to the north-west
 “ with a very great swiftness, and very high in
 “ the air. When it came opposite to the gabel-
 “ end of the dwelling-house, I then discovered its
 “ true figure : it was perfectly round at the great
 “ end, which went foremost, and tapered three or
 “ four yards, to my imagination, in length †. Being
 “ resolved to see it as long as I could, and fearing
 “ the wall and roof of my house might intercept the
 “ view (for I was then standing in the threshold), I
 “ moved six or seven yards farther off the house,
 “ keeping my eye fixed upon the meteor, and ob-
 “ served, that it had not gone above a quarter of a
 “ mile, when one-third towards the small end broke
 “ off; which third separated into sparks of fire, resem-

appeared to him when at the highest above the horizon; his
 answer was, about 12 : hence the apparent diameter of the meteor,
 to this observer, was not above one half, or at most two thirds, of
 the apparent diameter of the full moon at its greatest height. But
 whereas this person saw the meteor first, when it could not be
 much farther advanced to the northward than Cambridge, the ap-
 parent magnitude must have been considerably increased by the
 time it came opposite to the gabel-end of his house, where it was not
 at above the third part of the first distance. If therefore we allow,
 that when seen at the largest at this station, the diameter of the head
 was only equal to the apparent diameter of the moon at the same
 height, we shall find the real diameter of the meteor to have been
 about half a mile, upon computing its distance from the village of
 Ancram.

† Having afterwards inquired, whether the head was of a glo-
 bular form, distinct from the tail, he answered, “ that he could be
 “ assured, that the appearance was as in the figure; and that the
 “ head and tail formed one tapering stream of light.”

“ bling

“ bling stars, and immediately vanished. Soon there-
 “ after the remaining body vanished also, directly to
 “ the north-westward of my house, and the former
 “ darkness returned *. At this time, I imagined I
 “ still saw the intire figure of the body in the air,
 “ tho’ perfectly black ; but I have been since told,
 “ that this appearance might have been only a de-
 “ ception, occasioned by the brightness of the body
 “ striking the eye ; as when we first look at the sun,
 “ and then turn our sight to the ground, or a wall,

* I also inquired of the observer, by Mr. Smith, whe-
 ther the body went on for some time in its progress northward,
 after the separation of part of its tail, or instantly vanished ? The
 answer was, “ That the tail (meaning the part which broke off)
 “ went into sparks, and instantly vanished ; that some sparks came
 “ from the body also ; but that it went forward a little way before
 “ it was extinguished ; viz. so far as that he could be assured it bore
 “ then due north-west of him.” Upon this information, in order
 to know the place of extinction, I drew on the map a line north-
 west from Ancram, till it intersected the line of direction of the
 meteor, and found, that this point, by Elphinston’s map of Scot-
 land, lay near Fort William ; by Moll’s map of Great Britain,
 the intersection was carried as far to the northward and westward as
 the west end of the isle of Skye : but as I relied most on the former, I
 have referred the extinction to a point perpendicularly above Fort
 William. As for the separation of the tail, that other remarkable
 period, according to Mr. Pringle’s measures, it must have happened
 when the meteor was vertical to the southern and western part of
 the shire of Lanerk, near the borders of the shire of Air, as was
 observed before.

With regard to the apparent variation of the height, to the ob-
 server at Ancram, Mr. Smith wrote as follows, “ You inquire,
 “ whether, at the first sight James Turnbull had of the meteor, it
 “ appeared to be as high as it did afterwards, when opposite to his
 “ house ? His reply is, that, at first view, it appeared indeed
 “ lower in the air than it did afterwards, which he ascribed to the
 “ greater distance ; but that he cannot say, that, at the end of its
 “ course, it was certainly lower than when he saw it first.”

“ we

“ we fancy we still see the figure of the sun, but of a
 “ dark colour. Upon going into my house, I looked
 “ at my watch, and found it five minutes after nine.
 “ The time of the meteor’s appearance, during my
 “ observation, might be near a minute. After I had
 “ been in the house about five minutes, I heard a
 “ noise, like a clap of thunder, of some continuance;
 “ and, upon my daughter’s saying, there is thunder,
 “ I said, that could not be; for that I had seen no
 “ clouds when I was out. Upon this, I went out
 “ again, and found no clouds, but clear star-light.
 “ Several of my neighbours in the village of An-
 “ cram (which lies about 300 or 400 yards from
 “ me, a little westerly of the south, and over the
 “ middle of which the meteor passed, according to my
 “ imagination), likewise saw the meteor, and heard
 “ the report. One of them in particular says, that
 “ the noise came from the fire as it went along; but
 “ I cannot be persuaded of this, for, during the time
 “ of the light, I did not hear the least hissing sound,
 “ nor a noise of any kind. Another of my neigh-
 “ bours in that village, who heard the report, said,
 “ it sounded to him like a crashing noise, and in such
 “ a manner as made him imagine, that the gabel-
 “ end of his own and his neighbour’s house, which
 “ were contiguous, had fallen down at a time. I
 “ have been told, that the continuation of the noise,
 “ heard by our family, might have been owing to
 “ the particular situation of the house. It stands in
 “ a hollow, near the brink of the Ale, greatly over-
 “ topt by very high banks, partly of rock, partly of
 “ clay, and very steep, which lie along that river.
 “ Upon the top of these banks, to the westward of
 “ my

“ my house, and farther up the stream, is a large
 “ plantation, and it was directly over those trees that
 “ the meteor disappeared. In this case, the sound
 “ must have come down some part of the trough
 “ formed by the banks of the river; and it is be-
 “ lieved, that the continuation of it might have
 “ been occasioned by an eccho from the rocks, and
 “ the ruinous walls of an old monastery, which
 “ stands on the other side of the water, just opposite
 “ to my house: whereas the village of Ancram lies
 “ off the trough of the river on a rising ground, on
 “ a level with the plantation, over which the me-
 “ teor seemed to break. With respect to the height
 “ of that body, all I can say is, that I thought it
 “ very high, and can be positive it was nearer what
 “ Mr. Pringle calls the zenith than the horizon. I
 “ have pointed out the place to that gentleman, as it
 “ appeared to me; and, as he is to take the height
 “ of it with an instrument, he will be able to inform
 “ you more exactly.”

XVII. Before I received this letter, Mr. Pringle
 had sent me the figures of the meteor, which James
 Turnbull refers to. They were drawn by Mr. Smith,
 in presence of the observer, and by his direction. *See*
Fig. 4.

A B represents the meteor intire, after it had
 come fully in view. C D represents the same,
 after the separation. C E the third part of the
 tail separated. D F the head, with the remain-
 ing part of the tail. G the sparks of fire issuing
 from the part of the tail separated. H the
 sparks

sparks emitted from the remaining tail, where the separation was made. W the meteor reduced again to a round form, after losing its tail.

Here it may be proper to observe, that, with regard to the sparks, some part of them are marked in the figure somewhat different from what they are described in James Turnbull's letter. There he only says, that the third part of the tail, which was broken off, separated into sparks of fire; without taking notice of the sparks that likewise issued from the remaining part of the tail, which however was a circumstance he mentioned to Mr. Smith, when that gentleman drew the figure.

Some time after this letter, Mr. Pringle acquainted me, " That he had been at the farmer's house; that " he had surveyed all the places mentioned in his " letter; and, to make the whole perfectly clear, he " had drawn a plan, containing the apparent course " of the meteor, its apparent intersection with the meridian, and the bearings of all the places mentioned " in the observer's letter." *See the same Figure.*

K the farmer's house. L the mill. M N the meridian line. P the shepherd's house, over which the meteor first appeared, making an angle M K P of $42^{\circ} 20'$ with the meridian. V the hill and trees, apparently over which the meteor broke, making an angle M K V of $92^{\circ} 30'$ with the meridian. The line P V the horizon. O the meteor seen first in a round form over the shepherd's house. T the meteor beginning to appear in its proper form. P Q R U the path of the tail broke off over the trees.

These

These figures of the meteor are made much larger than they ought to be, in proportion to the other objects, in order the better to represent its true form.

After making this survey, Mr. Pringle got the farmer to point to that part of the heavens to which he referred the meteor, when opposite to the gable-end of his house; and the observer seeming to be well assured of the place, Mr. Pringle took the altitude with an instrument, and found, after three trials, the height to be about 58° *. He concluded with saying, "That, in answer to some more queries of mine, the farmer had told him, that he had observed little rising or falling of the meteor during its whole course; but that its motion, from the time he first saw it, to its extinction, seemed to be nearly in one straight line, at an equal height above the horizon †; and that the light was continued and uniform, without any fresh burstings of flames from either the head or the tail."

XVIII. All the information I received from that part of the country, over which the meteor seemed to break, was from Lord Auchenleck, one of the judges in Scotland, whose lands lie in the shire of Air, bordering on the shire of Lanerk. That gentleman was then at Edinburgh; but was so ob-

* At this time the meteor must have been vertical, about two or three miles to the southward of Lochmabin, a town in the shire of Dumfries distant from the observer about 37 miles, and from that place where the tail afterwards broke off 31 miles. From the altitude given here, I have computed the real height, at this place, to have been about 59 miles.

† This remark must be corrected by the last paragraph of the last note of Obs. XVI.

liging, as, at my request, to make all the inquiry he could among the people on his estate, and his neighbours; but could procure me no other account than what is contained in the following letter, which he had from his gardener. “ In obedience to
 “ your lordship’s orders relating to that great meteor,
 “ which made its appearance upon the 26th of November, 1758, as I did not see it myself, I cannot
 “ give such a particular account of it as I could wish;
 “ but what I can collect from sundry folks in this
 “ neighbourhood, is as follows. About a quarter
 “ after nine that night, there appeared from the
 “ south-east a very great illumination or light, which
 “ instantly made such a splendor, that, to a considerable distance, one could most distinctly see houses,
 “ trees, water, stones, &c. but could not observe
 “ any particular body from which the light issued,
 “ nor that it ran farther westward; from which we
 “ may conclude, that it had then broke. No noise
 “ was heard, and no such thing as any ashes were
 “ found, that I could hear of. During the preceding part of the day, we had a strong and very
 “ cold south-east wind, with a little frost; but the
 “ evening was more calm.”

Signed, *James Bruce.*

From this letter it appears, that the sky in those parts, as about London, was then so much clouded, as to hide the body of the meteor, tho’ the light of it was very manifest, and which, I presume, was the brighter there for the bursting of the tail, and its dissolution into sparks of fire, when almost vertical to the observers.

XIX. Sir

XIX. Sir Robert Pringle, who was at Sutchill, (about 10 miles N. N. E. of Jedburgh, and about 60 miles, nearly in the same direction, from Carlisle), wrote as follows. “ I did not see the meteor you mention, “ nor have I yet met with any body that observed “ it, further than the great light with which it was “ attended, making every thing to be seen on the “ ground as distinctly as in sun-shine, and which “ continued, as they said, much longer than a com- “ mon flash of lightning from thunder. At that “ time I happened to be sitting, with some of my “ family, in the parlour, and all of us heard a “ noise we could not account for, as sounding “ like a gun fired off in the garrets, or a cannon “ discharged about a quarter of a mile from us ; but “ the noise continuing like thunder at a distance, we “ concluded it was nothing else, till one of the maid- “ servants came in, and told us, she had seen a very “ surprising flash of lightning, both for its clearness, “ which she compared to noon-day, and for its con- “ tinuance ; but she did not hear the report, which, “ I suppose, was occasioned by the noise of her own “ feet : for all the rest of our servants, that were sit- “ ting, and several other persons in the neighbour- “ hood, heard it much as we did. Some of our “ Edinburgh news-papers describe the body of that “ meteor to have been like a large star coming from “ the southward, and ending in the northward, both “ points westward of the observer, with a train after “ it, in form like a cone ; and with several sparks “ falling from it as it went along. These accounts “ say nothing of the length of this luminous appear- “ ance ; but that it seemed to be about 10 or 12 “ inches

“ inches broad at the head ; nor do they mention
 “ any sound that was heard after it vanished. A
 “ gentleman from Berwick told me, that he had
 “ spoken with a master of a trading vessel there, who
 “ saw this meteor of November last, as he was sail-
 “ ing in the Baltick, and in the same form it ap-
 “ peared in this country ; but did not mention its
 “ height, nor direction.”

XX. Mr. Redpath, son of Mr. Redpath of Angelraw (a place about four miles north-east of Stittchill, in the shire of Berwick), says in a letter, dated from his father's house, “ That he did not see the
 “ meteor himself, nor had found any intelligent per-
 “ son who had seen it throughout its whole course ;
 “ but that the best account he could procure was
 “ from one Mr. Mack, a farmer in the neighbour-
 “ hood, tho' he too had only observed it a little before
 “ its disappearance. That, from all he could gather,
 “ it was seen on Sunday the 26th of November,
 “ about nine at night ; its direction was from the
 “ south-east to the north-west (but nearer the south
 “ and north points than the east and west), with a
 “ tail of a considerable length, pointing down-
 “ wards *, inclining to the east ; that its course
 “ seemed to be very quick, and that sparks of fire
 “ fell from it as it moved along ; that the whole was
 “ of a conical figure, and appeared to be about five
 “ inches at its basis ; that a very strong light issued
 “ from it, which, in those houses where the candles

* This circumstance of the tail pointing downwards, is contrary to the other observations, and must have been a deception.

“ were out, darted thro’ the windows with such
 “ strength, that the rooms were wholly illuminated by
 “ it for 7 or 8 seconds; that its first appearance was
 “ not exactly at the horizon, but a little above it *,
 “ and that, at its greatest height, it certainly did not
 “ exceed 40 degrees; that it was extinguished before
 “ it reached the horizon, perhaps by about 8 or 10
 “ degrees †; that the colour of the meteor was at
 “ first nearly of a pure white, but, in proportion as
 “ it advanced, it grew red, and seemed to go out all
 “ at once; that the light, which issued from it,
 “ seemed rather to consist of successive flashes from
 “ side to side, than of an uniform regular flame; that
 “ a few minutes after its disappearance (not above
 “ three or four) was heard by several people a violent
 “ thunder-clap, or something very like it, and from
 “ the same point it disappeared, viz. rather nearer to
 “ the north than the north-west.”

XXI. At Dalkeith (a market town 6 miles south-east of Edinburgh), a gentleman, who happened to be walking eastward in the street, perceiving his right side and arm strongly illuminated, suddenly turned his face to the light, and saw the meteor, “ then in

* By Obs. XVI. the first appearance must probably have been a good way above the horizon; but Mr. Redpath says, he had met with no person he could depend upon for the account of the first appearance.

† Angelraw being, within a few miles, of the same distance from Fort William as Carlisle, we have, by this last circumstance, a confirmation of the real height of the meteor at its extinction, upon comparing with this observation the notes of N^o II. and N^o IX.

“ a direction at right angles with the street †, having
 “ an altitude, as he conjectured, of about 45 degrees;
 “ he observed that the figure was oval, the light great,
 “ and of a blueish cast; but he heard no sound.”

XXII. By an article published in the Edinburgh news-papers, “ the meteor appeared there of a conic
 “ form, about 5 or 6 inches broad at the basis, and
 “ lasted 5 or 6 seconds; its light was great, and
 “ sparks flew from it like those of a rocket, when its
 “ force is spent.” I wrote to Dr. Whytt, Fellow of
 the Royal Society, for more particulars from Edinburgh; but he returned for answer, “ That he had
 “ not seen the meteor himself, nor any body that
 “ had made proper observations upon it; the rest
 “ could only tell they saw a ball of fire, which emit-
 “ ting several sparks in its progress, moved quickly
 “ along the south part of the hemisphere, and then
 “ disappeared.” He added, “ that, on the night
 “ preceding that on which the meteor was seen, he
 “ had observed a very bright *aurora borealis*.”

XXIII. One of my friends acquainted me from Edinburgh, that the article in the Glasgow paper was to this purpose. “ About nine o'clock last Sunday
 “ night (viz. on November the 26th), a globe of fire
 “ came over this city from the southward, in ap-
 “ pearance as large as the full moon. It made the
 “ streets as light as at noon-day, lasted about a mi-
 “ nute, and, just before it vanished, it divided into

† The street, to the best of my remembrance, lies nearly north-north-east and south-south-west.

“ three parts directly over the middle of the town,
 “ and then ascended through the atmosphere *.”
 Mr. Smeaton, Fellow of the Royal Society, happened
 to be that night at Glasgow; but did not see the
 meteor, nor staid long enough to get any tolerable
 account of it. From the information he had, he
 judged it was not so vertical as the news-writer of
 that place has represented it.

XXIV. From Dunfermline (a town in the shire of
 Fife, about 14 miles north-west of Edinburgh), Dr.
 Stedman acquainted me, “ That he had only found
 “ two persons who had seen the meteor, a man and
 “ his wife, from whom he had the following parti-
 “ culars. That the figure was such as was delineated
 “ in the paper (*Fig. 5.*) which he sent me; that the
 “ first view they had of it was in the south-south-
 “ east, as it came from behind a building; and that
 “ it seemed to them to move westward; that the
 “ hinder part or train emitted large sparks or globules
 “ of flame, such as are seen to fall from a sky-rocket,
 “ when it begins to break; that its altitude was
 “ about 24° , which he had taken with an instru-
 “ ment, upon their shewing him how it seemed to
 “ move along the roof of a church, after bringing
 “ him to the window where they stood to see it;
 “ that they lost sight of it before it was extinguished,
 “ by a steeple that stood in the way; that its head

* I desired a gentleman at Glasgow to ask the writer of the
 paper, what he meant by this last expression; but he received no
 satisfactory answer, and could furnish me with no better materials
 from that place.

“ or fore-part appeared somewhat broader than the
 “ full moon ; that no sound was heard after its dis-
 “ appearance ; that the time was about nine at night,
 “ on Sunday the 26th of November *.”

XXV. In

* When Dr. Stedman sent me this account, he had omitted taking the bearings ; but, in his next letter, he told me, “ he had supplied that defect, and found, that the first appearance to the observers (when the meteor came from behind the building that intercepted the sight of it), had been about south by east $\frac{1}{2}$ east ; and that it had disappeared behind the steeple at about south by west $\frac{2}{3}$ west ; that, during this short course, it neither seemed to them to ascend nor descend.”

By Moll’s map, the first of these bearings intersects the supposed tract of the meteor in Westmorland, about 14 miles west of Appleby, distant about 102 miles from Dunfermline ; but cuts the tract so obliquely, that a very small error in the bearing, or in the path of the meteor, would make a considerable difference in the following calculation. The other bearing cuts the tract near the south-west corner of the shire of Lanerk, distant about 46 miles from Dunfermline.

The above measures being in miles of 60 to a degree, give the height of the meteor, when first seen at Dunfermline, to be about 54 statute miles, and at the last bearing to be about 24 statute miles, upon making allowance for the curvature of the earth. I suspect, that the observers here made the apparent altitude too low ; but, however that may be, from hence the dipping or obliquity of the course downwards is manifest.

It was said, that the head was somewhat broader than the full moon. Let us give some allowance to the imagination, and suppose the head was but equal to the full moon, and that only when the meteor was at the nearest. In this case, as the distance of the moon from the earth (about 240000 miles) is to the real diameter of the moon (about 2180 miles) ; so was the distance of the meteor (about 59 miles) to its real diameter : which will thus be found to be about half a mile. But if the apparent diameter of the head at its first appearance (that is, at the greatest distance from the observers), was equal to that of the full moon, then the real diameter of the meteor was about $\frac{2}{3}$ or $\frac{1}{2}$ part more than a mile. If it was indeed somewhat larger than the moon, then the real diameter might have been

XXV. In January after, I saw here one Mr. Cairns (a young man then appointed surgeon's mate to one of the regiments at Gibraltar), who told me, " That he had seen the meteor, of the 26th of November, " about 9 o'clock at night; that he was then in the " shop of Mr. Oliphant, surgeon-apothecary at Cul- " rofs (a town about 19 miles W. N. W. of Edin- " burgh); that Mr. Oliphant and he were surpris'd " by a sudden glare of light from the street, coming, " as it were, in successive flashes, but without any " intervals of darkness; that they both ran out, and " observed a ball of fire moving, with great velocity, " in a direction nearly from the south-east to the " north-west; that its height seemed to be consider- " able; but that they had not seen it to the last, by " reason of some houses on the north side of the " street, which stood in their way; that it was some- " what of a less size than that of the full moon, when " about the same height above the horizon; and of " an oval figure, with the longest diameter in the " course of its direction. He observed no tail, nor " sparks of fire issuing from it; but said, that some " people of the town had taken notice of the latter; " that the meteor itself was of a reddish fiery colour, " though the reflection of the light from the streets " was of a yellowish cast; that he heard no explosion " himself, and had met with none who pretended to

been about two miles; which is the length resulting from the obser-
vation made at Dublin, as will afterwards appear. But, in such
cases, the most moderate computations are most likely to be true;
so natural is it for the imagination to magnify all objects that
alarm it.

“ have heard any noise, either during the appearance of the body, or after its extinction.” Having inquired how long the light continued, Mr. Cairns answered at first, “ he believed about a minute or two;” but, upon looking at a watch, which had a second-hand, and desiring him to recollect the time from the first glare of light till the return of darkness, he stopt me when I had counted 13 seconds*.

XXVI. I wrote to Dr. Simson, professor of medicine in the university of St. Andrews (which lies about 31 miles N. E. by N. of Edinburgh), who answered, “ That his family had been alarmed by the light, and that one of them cried out, the heavens were all on fire; that his son (a minister)

* This account of the time is most likely to be the nearest to the truth; since, without examining it in this manner, those, who are unaccustomed to measure such small portions, will generally reckon it much longer than it really is. I have been confirmed in my opinion about the shortness of the time, by the observation of another gentleman, who, being that evening at a friend's house in the shire of Haddington, saw the light, without seeing the body. I desired him to recollect the time, whilst I counted it, and in three trials, he stopt me pretty exactly at 8 or 9 seconds. He imagined he could not lose above 2 or 3 before he saw the light. Mr. Dutton, watchmaker in Fleet-street, who, since this paper was first presented to the Society, happened to see another meteor, which had a course little shorter than the other, told me, that he could be assured, it was over in about four or five seconds; tho' he believed that others, not of his business, nor used to compute by small portions of time, would readily have assigned a minute or two for the duration of the appearance. But I shall suppose Mr. Cairns's measure, corrected by the watch, to be just; and therefore, as the meteor, in 13 seconds, passed from Cambridge to Fort William, a space of about 400 miles, it must have moved at the rate of about 30 miles in one second of time.

“ hap-

“ happened, at the first appearance of the light, to
 “ be standing close by a south window, and saw the
 “ meteor like a ball of fire, but of an oval figure,
 “ with its longest axis in the direction of its course, of
 “ a size equal to that of the full moon at her greatest
 “ height* ; that it moved, with great velocity, from
 “ the south-east † (about which point he first saw
 “ it) towards the N. W. but that he had lost sight
 “ of it about the S. W. by the intervention of a
 “ building on the opposite side of the street, be-
 “ fore it had fallen from its apparent height: that
 “ he observed no tail, nor sparks of fire issuing from
 “ it; and heard no noise after the return of darkness.”
 The doctor added, “That he himself was from home,

* If Mr. Simson lost sight of the meteor duly S. W. of him, it must have then been perpendicularly over the southern part of the shire of Lanerk, about 66 miles from the observer, and about the highest he could have seen it any-where in its course. I shall therefore suppose, that it was at its greatest apparent diameter just before it disappeared; that is, equal to that of the full moon, according to his comparison; consequently its real diameter was about half a mile, upon the like computation with that in the note upon Obf. XXIV. This is the most moderate; for the meteor might have been considerably larger even from this observation.

† Having omitted desiring Mr. Simson to take the bearings with a compass, he has not imagined that I required any greater precision than having the most common points; but as I find Cambridge laid down in all the maps nearly S. S. E. of St. Andrews, and as we have no reason to believe the meteor was lighted to the eastward of Cambridge, it is probable Mr. Simson did not see it till it was nearer to the south than the S. S. E. But supposing this gentleman saw it at its first setting out, viz. over Cambridge, and duly S. S. E. then, from the angle of elevation of $15\frac{1}{2}$ deg. the distance between the two places, and an allowance made for the curvature of the earth, the perpendicular height at Cambridge must have been about 100 miles.

“ at some miles distance, at supper, in a room with
 “ the windows darkened; so that he neither saw the
 “ light, nor heard any sound.” In a second letter,
 he acquainted me, “ That his son, at my request,
 “ had gone with a friend to the same window men-
 “ tioned above, and, pointing an instrument to that
 “ part of the heavens he recollected to have seen the
 “ meteor in, they had found the apparent altitude,
 “ during the time Mr. Simson saw it, to have been
 “ about 15 degrees and a half. As for the whole time
 “ of its continuance above the horizon, all the ac-
 “ count he could give was, that the body was not
 “ visible to him longer than he could have counted 6
 “ or 7 strokes of his pulse; but believes, if he had
 “ stood at a higher and an open window, he might
 “ have seen it much longer.”

One of my friends did me the favour to write to
 Bamf and Inverness, in order to learn what had been
 observed in the more northern parts of Scotland; but
 found the meteor had not been taken notice of at
 either of those places.

XXVII. Believing there was a better chance for
 hearing of its course more to the westward, a gentle-
 man here was so obliging as to procure me the follow-
 ing letter, written by Dr. Alexander Mackenzie, phy-
 sician in the shire of Ross, to a friend of his in the
 same country, on the occasion of this inquiry. “ I am
 “ sorry that the information I can give you about the
 “ meteor, of the 26th of November last, will be so
 “ little satisfactory: however, I shall tell you what I
 “ saw. I must first observe, that where I then was,
 “ viz.

“ viz. at Flowerdale (a gentleman’s house on the
 “ western coast of Rosshire), the view of the heavens
 “ is extremely confined, being quite surrounded, ex-
 “ cept at one point, by very high and close-approach-
 “ ing hills; whence you will understand, that the
 “ meteor must have been high before it could be ob-
 “ served, and that it quickly disappeared, as its pro-
 “ gress was very rapid. Its light was most surpris-
 “ ingly splendid, but not in the least like that of
 “ the sun, except when it shines through a cloud,
 “ or a summer shower. Its magnitude was near to
 “ that of the full moon, when she is three or four
 “ hours high. Its colour not at all like that of the
 “ body of the sun, or an ignited globe, but resem-
 “ bled that of the flame of spirits. Its figure was
 “ quite spherical, without any tail; but it emitted,
 “ or as it were dropped, sparks of various colours
 “ and magnitudes. As for its height, it was vertical;
 “ and its direction was from the west northerly to
 “ the east southerly. I was sensible of no noise
 “ on its disappearance. The time of night was
 “ about nine, and indeed as dark a night as ever I
 “ saw.”

Upon reading this letter, and finding, by Dr. Mac-
 kenzie’s observation, the course of the meteor to have
 been so very different in those parts from what I had
 collected from the other accounts, and believing it
 was beyond all chance, that a new meteor should
 appear at the same place, on the same day, and
 at the same hour when the other was expected, I
 doubted I had either misunderstood that gentleman’s
 expression, with regard to the direction, or that he
 possibly might have made a mistake in the writing ;
 and

and therefore, to be clear in this circumstance, I wrote to the doctor, desiring to be informed, whether he meant to say, as I understood him, that the course of the body was *from some point a little northward of the west, to some point a little southward of the east*; or otherwise. To which letter Dr. Mackenzie obliged me with this answer. “ Altho’ I regret my being out
 “ of the way of answering your letter in course of
 “ the post, yet, by that absence, I have it now more
 “ in my power to be exact with regard to some of
 “ your queries, as I am just returned from Flowerdale; where, in consequence of the first letter on
 “ the subject of the meteor, I observed narrowly the
 “ situation of the mountains over which it passed;
 “ in order that I might correct my account of its
 “ course, if I had mistaken it before. But, after
 “ that survey, I found my former description exact to a tittle, and your interpretation of my words,
 “ viz. *from the west northerly to the east southerly*, to
 “ be precisely what I meant: they may not be terms
 “ of art, but express the true progress of that body
 “ when I saw it; notwithstanding that I observe, by
 “ your informations from the south of Scotland, and
 “ from Carlisle, its direction was almost directly opposite. What I meant by its vertical height was,
 “ that its declination, if any, was extremely small
 “ from the zenith, but that northerly. Tho’ I continued to gaze for about a quarter of an hour after
 “ it disappeared, I was sensible of no sound, neither
 “ like that of thunder nor a cannon: yet such might
 “ have been in a lesser degree without my hearing it;
 “ as the noise of the sea that night was remarkably
 “ louder, than at any other time, during the whole
 “ month

“ month I was at that place. I can positively assure
 “ you no tail was visible there; tho’, as I said before,
 “ or at least meant to say in my former letter, the
 “ meteor emitted or dropt a great many sparks of va-
 “ rious magnitudes, and most beautiful colours, some
 “ of which seemed to equal the size of half a crown.
 “ My wife and another lady, at fifty miles distance,
 “ almost due east from Flowerdale, saw many such
 “ sparks, but no part of the body of the meteor. The
 “ light, tho’ of the pale moon-colour where I was,
 “ yet was so bright, that I could discover every bush
 “ and tree, every scraggy rock on the tops of the
 “ mountains, altho’ the night, both before and after
 “ its disappearance, was extremely dark, and with-
 “ out a star to be seen. I am not a little surprised
 “ that, considering how early in the night the meteor
 “ made its appearance, not one person, besides my-
 “ self, in all this country, as far as I can learn (and
 “ I have taken pains to inquire), happened to see it;
 “ except you will admit that my wife and her friend
 “ saw some of its tract, from the sparks they ob-
 “ served. And this circumstance leads me to think,
 “ that it made a very quick turn immediately after
 “ its disappearance from my sight, losing its south-
 “ ern direction, and running due east: which, in my
 “ opinion, confirms the ship-master’s report of see-
 “ ing it in the Baltick.”

Thus far Dr. Mackenzie, who, I am persuaded,
 could not be mistaken about the points of the com-
 pass, in a part of the country he is so well acquainted
 with, nor would offer any such account of a fact
 he was not well assured of: so that, upon the
 whole, we must refer this strange curvature in the
 course

course of the meteor to some principle, at first view, very different from the common laws of motion ; but perhaps not altogether inconsistent with them, as I shall endeavour to shew in my next paper *.

I finished my inquiries for the north, by writing to a friend in the isle of Shetland ; but he had heard nothing of the meteor there.

XXVIII. I shall conclude with one account more, which I had from Dublin, in a letter from Mr. Cleghorn, author of the natural history of Minorca. I imagined, that a tolerable observation from that quarter would be useful, for ascertaining both the height of this body above the earth, and its real magnitude ; and accordingly I received from thence some proper materials for that purpose. Mr. Cleghorn writes, “ That altho’ the meteor, of the 26th of November, “ did most certainly appear at Dublin, as well as in “ England, yet few people had observed it with at- “ tention, and none, that he could hear of, had com- “ mitted any thing to writing, excepting one Mr.

* It seems, from observations made on other meteors of this kind, that the curvature in the path of such bodies is not extraordinary. Thus, one that appeared in Italy in 1719, recorded in the first volume of the Academy of sciences at Bologna, did not pursue its course in a straight line ; for they say, *directio non eadem semper fuit*. Again, in the history of the Royal Academy of sciences at Paris, for the year 1738, we find another mentioned, which had so crooked a motion, that they call it *un mouvement bizarre*. Lastly, I observe, that one of the gentlemen, who gave an account to the Royal Society of the meteor seen, about London, in the year 1741, describes it as first shooting to the north-east, and afterwards to the south-east. See *Philos. Transf.* N° 463. p. 59. *Abr. Vol. VIII. p. 525.*

“ Garret, a good sensible man, with some mathematical learning, whose account he had therefore sent me, in an extract from a register of the weather kept by that person; and to which was subjoined an answer to some queries, that had been put to him, concerning that body.” Here follows the paper.

Extract from a register of the weather, by Thomas Garret, inn-keeper at Island-bridge, near Dublin.

“ November the 26th, 1758, hard blowing weather; wind at south-east. Fifteen minutes past eight in the evening *, a globe of fire about seventeen degrees above the horizon †, due east, moved from south to north, as large in appearance as the moon, but more of a golden colour; it broke and dispersed, like a starry rocket, in small, bright sparkles, nearly before the wind, or as if they passed away with the wind.

* By the difference of longitude, this time answers to about forty-one minutes past eight at Cambridge, about half an hour after eight at Carlisle, about 32 minutes after eight at Ancram, about 26 minutes after eight at Edinburgh, and about 20 past eight at Flowerdale in Rosshire.

† By Moll's map, Dublin is distant from Cambridge about 240 miles (at 60 to a degree); and a line drawn due east from Dublin, cuts the tract of the meteor near the north-east corner of Derbyshire, distant by the same map about 185 miles from Dublin. From these measures, and the apparent height, the real altitude of the meteor at Cambridge must have been about 95 statute miles, and over the corner of Derbyshire about 72 statute miles, allowance being made for the curvature of the earth. This observation, compared with Obs. II. and XXIV. with the notes, makes the obliquity of the course very manifest.

“ *N. B.* Mr. Garret keeps his clock very exact,
“ by Glasgow’s regulator, Christ-Church.

“ Emanuel Miller, of Island-bridge, saw this meteor from beginning to ending ; thinks it was above
“ the horizon about half a minute ; and says, that it
“ moved with less rapidity than falling-stars commonly do.”

“ The following queries were put to Mr. Garret,
“ and his answers are annexed.

“ 1. Was it a perfect sphere, or an oblong ball ?

“ *Ans.* A perfect sphere.

“ 2. With or without a tail ?

“ *Ans.* Without a tail.

“ 3. Did any sparks of fire issue from it ?

“ *Ans.* There did not any sparks issue from it till
“ it broke.

“ 4. In what direction did it move, from S. E. to
“ N. W. or otherwise ?

“ *Ans.* It moved from S. towards the N *.

“ 5. How long did it continue visible ?

Ans. The observer says, he saw it only for the
“ space of three seconds, or the twentieth part of a
“ minute ; but that he is sure it had been visible
“ some time before, he having been called out of his
“ house by his servant on purpose to look at it.

“ 6. Was any sound heard like a clap of thunder,
“ or the report of a cannon, after its extinction ; and
“ how long after the disappearance of the light ?

* From that station, the declination to the westward of the north could not be well observed.

“ *Ans.* There was not any sound heard: it was
 “ supposed to be at too great a distance.”

These are all the accounts which I received, at different times, upon this subject. In another paper I shall offer to the Society some remarks, that have occurred to me, upon a careful review of the whole materials.

XXVII. *Some Remarks upon the several Accounts of the fiery Meteor (which appeared on Sunday the 26th of November, 1758), and upon other such Bodies; by John Pringle, M. D. F. R. S.*

Read Dec. 20, 1759. **I**N my last paper, I laid before the Society all the accounts I had received of the meteor, of the 26th of November, 1758; which I could not reduce into a narrower compass, without the hazard of omitting such observations, as might be judged necessary for ascertaining some of the principal circumstances; or without passing over those facts, which, however immaterial they may appear at present, might afterwards afford some light in explaining the nature of these *phænomena*. I have even inserted some particulars contradictory to others, that I imagined more likely to be true, as I myself might be deceived, and as, by preserving the several relations intire, I thereby did most justice to the observers. The deception of the senses, upon the appearance of unusual objects, the short continuance of the meteors, and

the surprize occasioned by them, at a late hour, among people unaccustomed to think on these subjects, will sufficiently account, not only for the variety, but the contradictions, in the several observations.

I come next to the result of all the relations, with regard to the true path, the velocity, figure, magnitude, and other circumstances relating to this body ; which having deduced in the best manner I could from the preceding materials, I shall now submit to the judgment of the Society. But as my last paper on this subject was of so great a length, I shall not farther insist on it now, than barely to mention the conclusions I have made, without losing time in recapitulating the several steps that led me to them *.

First then, as to the path. This meteor seems to have been vertical at Cambridge, or nearly so, and to have taken fire about the zenith of that place ; or at least to have appeared first there in a state of ignition. From thence it proceeded directly, almost N. W. by N. over several counties in England, over the Solway frith (which it crossed between Carlisle and the town of Dumfries) ; and in Scotland over the shires of Dumfries and Lanerk : but soon after its becoming vertical to the last, viz. a few miles to the southward of Douglas (or perhaps nearer to the borders of Lanerk and the shire of Air, about 10 or 12 miles to the eastward of Auchenleck), part of the tail seemed to break off, and to disperse in bright sparks of fire ; whilst the head, into which the remainder of the tail was instantly collected, moved on in the same

* Some of the most material are subjoined in notes to the former paper, since it was first presented to the Society.

direction,

direction, till coming over Fort William, in the shire of Inverness, after a course of about 400 miles, it there suddenly disappeared.

But, notwithstanding the extinction of the meteor at this place, it seems still to have proceeded northwards; since it was seen again, in a luminous state, in a globular form, but without a tail, about the 58° of latitude, on the western coast of the shire of Ross, almost vertical to the observer; moving then to the southward of the east; that is, in a direction almost contrary to the first: and in this last course, of which we know not the end, it possibly might have gone a great way to the eastward.

During the first part of its progress, viz. from Cambridge to Fort William, it went obliquely downwards, in such a manner, that, by computation, it must have been from about 90 to 100 miles high at the first of these places, and between 26 and 32 miles at the last. But at what height it was afterwards seen in the shire of Ross, is not to be determined, since one observation only was transmitted to me from that country. As Dr. Mackenzie observed it nearly over his head, and yet of a smaller size than the full moon when some hours above the horizon, I should suspect, that, after its descent to Fort William, it had re-ascended at the time he saw it; because he probably would have described it as a larger body, if then not higher than when it first disappeared.

This dipping and rising, in the course of a meteor, is not more extraordinary than its lateral deviation from a straight line, as I have elsewhere shewn*. Such

* See the note subjoined to Obs. XXVII.

are the facts, tho', perhaps, it may be hard to account for them from the common principles of motion. What seemed most likely to be the cause of the vertical change of its direction, was its rapid descent towards the earth, till it came so low as that the resistance of the air might act upon it; in which case, the lower part of the body meeting with denser air than the upper, it would be so reflected from that medium as to ascend; for tho', at the height of 20 or 30 miles, the air is extremely rarified, yet, in this instance, the immense velocity of the impingent body will make a less resisting medium produce a greater effect. The meteor, therefore, might be reflected by the air, in the same manner as a cannon ball by water, when it strikes it in a very oblique direction.

If this reasoning can be admitted, we may account for the lateral variation of its path, in the manner following. Although it should seem, that, during the first part of the course, the wind was at S. E. yet, when the meteor advanced towards the north of Scotland, it there probably met with a westerly wind; from this circumstance in Dr. Mackenzie's letter, *of the sea being that night remarkably louder than at any other time during a whole month he had been at Flowerdale*: for that place lies on the western ocean, in the shire of Ross; and there, I imagine, the sea could not produce that stormy noise, unless the wind had blown from some point between the west and the north. If therefore the current of the air obliquely opposed the motion of the meteor, that body would be gradually reflected into a new direction; from which, by another current, it might have been turned a second or a third time, until it fell at last into that path

path described by the doctor. From this account it will follow, that the lower a meteor descends, the more irregular its course will be; as it will then meet with the most resistance, both from the density and the currents of the air.

In regard to the velocity, it seems almost incredible; as we have sufficient *data* for computing it at the rate of 30 miles in a second. But if we allow, that it only moved through half the space in that time, we shall find the progression of this body to have been above 100 times swifter than the mean celerity of a cannon ball, and nearly equal to that of the earth in its orbit round the sun.

As to its real size, we cannot pretend to determine that point with any precision; since its dazzling brightness would occasion some deception, and the apparent magnitude has been so differently represented by the observers. If the meteor, when nearest to Dublin, appeared to Mr. Garret equal to the full moon, then we shall find, that its true diameter was about two miles; and if the farmer at Ancram saw this body, whilst it was vertical at Cambridge, of a size equal to the crown of his cap, or to about half that of the full moon, we cannot allow less than a mile for the real axis. Upon estimating from the observations made at St. Andrews and Dunfermline, the diameter was at least half a mile, and perhaps much greater. However, as the imagination is so apt to enlarge such objects, we shall put the size of the globe at the smallest, and reckon it only about a mile and a half round.

The body must surely have been of a considerable bulk to have yielded such a light, as that, when in the
zenith

zenith of Cambridge, the farmer at Ancram (at the distance of above 260 miles) should, upon entering his threshold, see the whole side of his house illuminated by it; and, to use his own expression, *with a brightness as of sun-shine*. And indeed the greatness of the light is every-where taken notice of, even at those places where the atmosphere was so thick as to hide the tail, nay, the whole meteor, as at Auchenleck, where it was nearly vertical.

As for the tail, it was a stream of light several miles in length; for this was no deception, like what we suppose the train of a shooting star to be, but was either a real flame, or, what is more probable, it consisted partly of flame, but mostly of smaller masses of fire (which the observers call *sparks*, when falling out of the lucid tract), and of vapours or fuliginous particles, not heated red-hot, but illuminated by the parts actually burning. Perhaps these vapours were the chief part of the composition, and which will account for its light being so much fainter than that of the head; since in some places where the air was less clear, or the distance greater, we find the whole meteor described either as a round ball, or a spheroid (with the largest axis in the direction of its motion), but without a tail. In this last case, viz. that of the oval form, it is probable that, besides the head, the beginning of the tail was also visible, as consisting of flame, and therefore brighter than the rest; and that both together appeared oblong to those observers. But such as were nearest, and had a clear atmosphere, saw the tail of a considerable length; that is, the flame, the sparks and the illuminated vapour, in a train behind the head, as being lighter,

and therefore meeting with more resistance from the air; in the same manner as the flame, the sparks, and smoke, of a torch are seen to follow it. All this is plain; but, in regard to that separation of the third part of the tail from the rest, a circumstance clearly described by the farmer at Ancram, and seemingly confirmed by other observations, there may be some difficulty. Perhaps at this period, upon a greater explosion in the ball, most of the combustible matter was thrown out at once, which falling behind, occasioned that appearance of the breaking off a part of the tail, whilst, for want of fuel, the remainder vanished, or, as the observer expresses it, *was collected into the head*. This account is rendered more probable, by what is said of the emission of a greater light about this time, and by the loud report heard by the farmer five minutes after, and which, upon computing the distance, ought to have reached him much about that time, had it been occasioned by this extraordinary bursting and dispersion of the inflammable matter.

The hissing noise, taken notice of by some while the meteor passed them, was a deception of that kind, which frequently connects sound with motion; and is the case of those who fancy they hear something, when they see the shootings of the *aurora borealis*; I say a deception, because if the meteor, during its course, really made any noise, so great was the distance of that body, and so short its continuance, that this sound could not have been heard till some minutes after the return of darkness. But the final report, so frequently mentioned, not only heard by those who saw the light, but by others who knew

nothing of what had happened, was a real sound, and immensely greater than any we are acquainted with. For, at the distance of 70 miles and upwards, it was compared to loud thunder, the report of heavy artillery, the fall of the gabel-end of the house the person was in, and to a musket fired off in the garret. If this noise was produced when the body threw out those masses of burning matter (by the observers called sparks of fire, the bursting of the tail, and delineated in *Fig. 5.* as balls of a smaller size in the train itself), we shall find, that at this time the meteor, by being more than 41 miles high, was in a region where the air is three thousand times rarer than on the surface of the earth; that is, about six times rarer than in a common exhausted receiver, where sonorous bodies are not heard, and even where gunpowder and the *pulvis fulminans* take fire, and are exploded, but without noise. Hence I would infer, that the separation of the elastic matter must have been performed with a velocity exceeding all imagination; as the intensity of sound so much depends on the resistance of the air, and as this elastic matter could fly off with so much celerity, as to find so great an opposition from so thin a medium*.

I should also conclude from the great report, that the substance of the meteor was of a firmer texture than what could arise from mere exhalations, whether formed into a sphere, and then burning; or dis-

* *M. Saluce* has lately shewn, by some curious experiments, that such substances as gunpowder and the *pulvis fulminans*, have a detonation in proportion to the rapidity with which the internal air is separated, and to the resistance of the external air. See *Miscell. Taurinens.* Tom. I.

posed into a kind of train, and consumed by a running fire: for sounds, as far as we know, are either produced by the quick and violent percussions of hard bodies upon the air; or by the sudden expansion of an elastic fluid, after being condensed within some solid substance. The noise occasioned by the motion of electrical matter is, perhaps, the only exception; but we have no reason to imagine, that this was at all concerned in the present case. There seems to be the more ground for believing this body was solid, at least that its surface was so, from finding, that, after the violent explosion, it still retained its form; a circumstance that could hardly take place, if the meteor had consisted of nothing but vapours. We may therefore presume, that the burning matter found vent through a hard crust by certain apertures, which either might have been there invisible, or unobserved. All I can say in support of this conjecture is, that, by the Memoires of the Academy of Bologna, we find a meteor appeared in Italy in the year 1719, lower in the air than that we have now been treating of, and in which, it is pretended, four several chasms were distinguished, each emitting smoke*. To these arguments for the solidity of this body, we may add its extreme velocity, and the intensity of the light; which are likewise circumstances more conformable to a heavy and solid substance, than to one formed of exhalations only.

Upon the whole, I believe it will appear, that these relations are not favourable to the prevailing

* *Apparebant in eo hiatus seu voragines quatuor fumum exhalantes.*
Instit. & Acad. Bonon. Tom. I. p. 285.

hypothesis about the formation of such bodies, which makes them to consist of certain sulphureous vapours arising from the earth: for, besides what has been urged above, Dr. Halley has shewn, “ That, at the height of 41 miles, the air is so rarified as to take up 3000 times the space it occupies on the surface of the earth; and that, at 53 miles high, it would be expanded above 30000 times: but thinks it is probable, that the utmost power of its spring cannot exert itself to so great an extension; and that no part of the atmosphere reaches above 45 miles*.” This being the case, how can we suppose any such vapours to rise to the height of 90 or 100 miles, where the air must be so many millions of times rarer than what we breathe? Again, we find that all vapours, in their ascent, mutually repel one other; so little do they seem disposed to run together into globes of such a size. But Dr. Halley observes (in treating of the great meteor of March 1719), “ That tho’ the aqueous, and most of the other vapours, are soon condensed by cold; and wanting a certain degree of specific gravity in the air to buoy them up, ascend but to a small height;” yet he adds, in favour of the opinion of these bodies being formed of exhalations, “ That the inflammable sulphureous vapours, by an innate levity, have a sort of *vis centrifuga*, and not only have no need of the air to support them, but, being agitated by heat, will ascend in *vacuo Boileano*.” From hence he infers, “ That the sulphureous vapours, disengaged from all other particles, may rise far above the reputed

* Phil. Trans. N° 181. p. 104. Abridg. Vol. II. ch. 1. § 9.

“ limits

“ limits of the atmosphere; and contracting them-
 “ selves into a narrower compass, by that principle
 “ of nature that congregates the *homogenea*, may lie
 “ like a train of gunpowder in the æther, till catch-
 “ ing fire by some internal ferment, the flame may
 “ be communicated to its continued parts, and so
 “ run on like a train fired,” &c *. But this *hypo-*
thesis of that excellent naturalist seems liable to the
 following difficulties. 1. What are the experiments
 which shew, that inflammable vapours have any sort
 of a *vis centrifuga*; or will rise either in an absolute
 vacuum, or in a medium where the meteors are often
 found, and where, according to the doctor, the air
 must be five hundred times rarer than in a common
 exhausted receiver †? 2. Altho’ sulphureous vapours
 may ascend from the earth, upon being agitated by
 heat, will not that volatility soon cease, by the cold-
 ness of the air, long before they can arrive at the up-
 per regions? 3. Does not all matter, capable of
 taking flame, suppose a proportion of an acid salt,
 adherent to the phlogistic principle, and consequently
 some gravitation? 4. And will not the weight be
 increased by the large quantity of condensed air, or
 some other elastic fluid combined with the inflam-
 mable vapours, which is the cause of the explosion
 and report? 5. Are we not led to this notion about
 the innate levity and *vis centrifuga* of igneous matter,
 from finding, that heat has a greater tendency upwards

* Phil. Transf. N^o 360. p. 978. Abr. Vol. IV. Par. II. Ch. 1.
 § 28.

† His expression is, *above three hundred thousand times rarer than
 what we breathe.*

than downwards? but is not this owing to that portion of the air, which, receiving the heat, and being thereby dilated, becomes specifically lighter than the rest, and therefore rises in the colder and more condensed atmosphere? 6. Suppose attraction to take place, should not those vapours, by that law, assume a globular form, and not, like a train, contract in breadth, and extend in length? 7. What is to set this train on fire; since a ferment, according to the chemists, implies a mixture of heterogeneous parts, contrary to the doctor's *hypothesis*? 8. How comes this train to be drawn out in so straight a line, and of so equal a breadth, inasmuch that, in the path of the late meteor, for about 400 miles, there was no sensible deviation, nor any change in the magnitude of the luminous appearance, but what might be accounted for from its greater or less distance from the observer, or its being seen through a clearer or an obscurer atmosphere?

Some have been of opinion, that these fiery meteors are only a kind of lightning, at greater heights than common; forming their notion upon the velocity of those balls of fire, and upon the sound accompanying them, so much resembling that of thunder. But this *hypothesis* having gained no credit, I need not employ time in refuting it; and the less now, as the nature of lightning is so much better understood than when this theory was first published. I shall only observe, that before the matter of lightning was discovered to be of the electrical kind, it was natural to suppose it to be formed of the sulphureous vapours arising from the earth; and if the earth was found proper for producing such exhalations, of
course

course it was judged capable of furnishing materials for all the lucid *phænomena* in the æthereal regions. Thus, not an hundred years ago, the comets themselves were accounted for on no better principle; and therefore we are the less to wonder if these meteors have been hitherto almost constantly referred, even by the best naturalists, to the same origin.

Of all the *hypotheses* that have come to my knowledge on this subject, a hint of Dr. Halley's (in a paper presented to the Society several years before the above-mentioned), seems best to agree with the late meteor; viz. that such bodies may be formed independent of any vapours from the earth*. But, with regard to the rest of his conjecture, that the one he described, "might be a collection of matter formed in
 " the æther by some fortuitous concurrence of atoms;
 " and that the earth had met with it newly formed,
 " and before it had conceived any great *impetus* of
 " descent towards the sun," I say, with regard to these circumstances, they are not at all supported by the present case: for, if we consider the immense velocity with which the late meteor moved northwards (and in judging of which we could not be deceived by either the diurnal or annual motion of the earth), we must be convinced that the earth did not meet with it before it had conceived a considerable *impetus* of descent, either towards the sun, or some other attracting power. And altho' there was likewise a motion of this body towards the earth, as appeared by the dipping of its path, yet it did not come quite down,

* Phil. Transf. N^o 341. p. 159. Abr. Vol. IV. Par. II. Ch. 1.
 § 25.

as might have been expected, had gravitation to the earth been the only principle concerned; but it descended no lower than to a certain depth in the atmosphere, after which it seems to have risen again, and made a considerable progress in the higher regions; contrary to Dr. Halley's opinion about such meteors, which, he believed, actually came to the ground *. And here I will venture to affirm, that, after perusing all the accounts I could find of these *phænomena*, I have met with no well-vouched instance of such an event: nor is it to be imagined but that, considering the frequency of such appearances, if these meteors had really fallen, there must have been long ago so strong evidence of the fact, as to leave no room to doubt of it at present. Their descent, under the horizon, is sufficient to make the common observers believe they see them come to the ground, whilst an explosion, high up in the air, coming late to their ears, passes for the crashing noise of the fall. Not that I call in question the possibility of their touching the earth; which they are likely to do as often as they move perpendicularly towards it, and not in that oblique direction so often mentioned, and by which means, it should seem, they are constantly reflected by our atmosphere. All that I would conclude is, that hitherto we have had no certain proof of their fall; and it is to be hoped, that their motions, like those of the comets, have been so regulated at first by a governing Power, that we have nothing to apprehend from their aberration. Unless we should imagine, that the perpendicular descent and bursting of

* Vide loc. cit.

one of these bodies has given rise to the story of Phaëton; since no other event will, perhaps, so well account for not only the foundation of that fable, but for a prevailing opinion among the ancients, that, besides a large tract of country, even the heavens themselves, in appearance, had once suffered by a conflagration.

If it is then probable, that these balls of fire come from regions far beyond the reach of our vapours; if they approach often so near to the earth, and so seldom or never touch it; if they are moved with so much celerity, as in that respect to have the character of celestial bodies; if they are seen flying in all directions, and consequently have a motion of their own, independent of that of our globe; if they part with such quantities of an elastic fluid, a phlogistic matter, and probably an acid, surely we are not to consider them as indifferent to us, much less as fortuitous masses, or trains of terrestrial exhalations in the æthereal regions; but rather as bodies of a nobler origin, possibly revolving about some center, formed and regulated by the Creator for wise and beneficent purposes, even with regard to our atmosphere; which, during their combustion, they may supply with some subtile and salutary matter, or remove from it such parts as begin to be superfluous, or noxious to the inhabitants of the earth.

Since these sheets were printed off, Dr. Pringle received another letter from Sir Robert Pringle, acquainting him, he had not found that part of his intelligence hold good, of the ship-master's seeing the meteor (of the 26th of November 1758) in the Baltick; but, upon further inquiry, had learnt, that though this person had observed an appearance of the same kind in those seas, it was not on the same day.

Dr. Pringle thinks it likewise proper to inform the public, that, since his paper was printed, he has received two further accounts of the meteor, one from Mr. Pringle, sheriff-depute of the shire of Roxburgh; the other from Mr. Garret, of Island-Bridge, near Dublin. Mr. Pringle says, "that having, since his last letter, drawn a more accurate meridian, at the farmer's house at Ancram, he now finds, that the shepherd's house (mentioned p. 236) bears nearly S. E. by S. from the farmer's dwelling house; that the hill with the trees, over which the meteor seemed to break (laid down in *Fig. IV.* as at $92^{\circ} 20'$ W. of the meridian), really bore W. by N. from the observer; and that the bearing of the luminous body, at the point of its extinction, was nearly W. N. W. and not due N. W. as the farmer at first imagined, who, upon this last survey, was convinced of his mistake." Upon this communication, Dr. Pringle drew, on two different maps, a W. N. W. line from Ancram to the path of the meteor, and found, that in Elphinston's map of Scotland, the intersection was a few miles to the eastward of Glasgow, but in Kitchen's map a little to the westward of it. In consequence of these last observations, the Doctor has fixed the point of extinction to the zenith of that city. But as Glasgow lies about 76 miles to the southward of Fort William, over which the meteor, in the preceding paper, was supposed to disappear, the first course of that body is reduced to about 324 miles in thirteen seconds of time, and its velocity therefore to about 25 miles in one second. Mr. Pringle has likewise acquainted the Doctor, "that the farmer, on seeing the engraving of the 4th figure (of which Mr. Pringle had received a copy), had found fault with the size of the head of the meteor, as being too large in proportion to the length of the whole; since, to his imagination, the diameter of the head did not exceed 8 inches, and that the tail seemed to be about 3 or 4 yards long."

Mr. Garret acquaints Dr. Pringle, "that, since his first letter to the Doctor, Emanuel Miller and he, having communicated their observations to Mr. William Gibson, mathematician, at Dublin, that gentleman had come to Island-Bridge, and having made a survey with his instruments, found, that the greatest altitude of the meteor was no more than 12 degrees."

ERRATA.

P. 240. *lin. penult.* for P Q R U, *the path of the tail broke off over the trees*, read Q R S U *the path. E F the tail broke off.*

In the plate, *E F the breaking of the tail*, should not have been represented as directly over the trees, but a little more to the southward, so as to bring the ball W (viz. the meteor at its extinction) perpendicularly over the said trees.

XXVIII. *Thoughts on the different Impregnation of Mineral Waters; more particularly concerning the Existence of Sulphur in some of them, by John Rutty, Doctor of Physic.*

Read Nov. 15, 1759. **I**NASMUCH as the existence of sulphur in waters hath been doubted, not only by Lister and Hoffman, but by another author, that has lately appeared, to whom the public has been, in some measure, indebted for exploding sulphur from some waters, on which it had been too liberally, and without the due evidence of experiment, attributed; I have therefore thought it worth while to review, collect, and sum up, the evidences of sulphur in waters, in order to shew, not only that antiquity hath not altogether rashly attributed sulphur to waters, but how far the existence of that mineral is demonstrable to sense in several, and more especially the cold, waters of that denomination.

1. That the feter of these waters is not owing to mere stagnation; and that they possess something more than what common water acquires by putrefaction, appears not only from Dr. Short's observation of some of these having a full and brisk current, but because putrid rain-water, and many of our chalybeate waters, turned putrid by keeping, do not discolour metals, as these waters do.

2. The effects of these waters, and their vapours, in discolouring metals, and their peculiar smell and flavour, like that of boiled eggs, and in the stronger like that of rotten eggs, are perfectly similar to those

of the artificial solution of sulphur, and its vapours.

3. Many of these waters, both foreign and domestic, are found to contain the native alkaline salt, which is the proper menstruum for, and has the same effect in, dissolving sulphur, as the artificial alkali; viz. not only the hot waters of Aix la Chapelle and Borsel, but the cold of Geronsterre; and, in England, those of Chadlington, Nottingham, Bilton, Quincamel, Suttonbog, and Wiggleworth; and the following in Ireland, viz. those of Swadlingbar, Derrylester, Lisblenk, Ashwood, Derryherce, Anaduff, Aghaloo, and one lately discovered at Lucan near Dublin.

4. Accordingly a milkiness, or incipient precipitation, analogous to *lac sulphuris*, is produced in several of these waters, by dropping acids into them; particularly in those of Aix la Chapelle, even according to Dr. Lucas's own testimony of the effects of distilled vinegar on it, and in that of Moffat in Scotland; of Harrigate; in our Swadlingbar water, and another of our springs of this sort in the C. Fermanagh: and to this add the white hairy mucus ordinarily precipitated on the sticks, or grass, in the passage of these waters, analogous to a magistery of sulphur.

5. The sulphur in waters is in a most highly attenuated, subtil, and fugitive, state; insomuch that, as Dr. Lucas observes of those of Aix la Chapelle, there is a great alteration in the colour of the precipitate caused by solution of silver in that, which hath been immediately drawn from the source, and that which has lain by only twenty seconds, even in a bottle filled, and close stopp'd; so soon is it lost or dissipated: and moreover, it is also blended with other minerals;

so

so that it is no wonder it should be difficult to exhibit a palpable sulphur, and that the distinguishing appearances proper to that mineral should often fail, particularly the above-mentioned test by acids, and the burning blue.

6. That a real sulphur, or bituminoso-sulphureous substance, is dissolved in these waters, and subsists in some of their less volatile or more fixed parts, is evident from the following appearances in the mud and scum collected from several of them: for the mud of several of the cold waters I have called sulphureous, as well as that of several hot baths in Germany and Hungary, mentioned by Browne in his Travels, is variegated with the several colours of yellow, green, and red, as the real sulphur, and, in some experiments, burnt with a blue flame, and a sulphureous smell; and the like evidences may be given of the sulphureous quality of the scum of divers of our cold waters, particularly in that of Mechan, in the north of Ireland, which, being dried, exhibited on the upper side a whitish yellow, or cream-colour; but underneath a deep grass-green, a pale, beautiful gold-colour, and a light reddish pink-colour, interspersed in a substance of a leaden blackish colour; every colour excellent in its kind, and as slippery as frogs spawn: varieties of colours, like these, being also found in the preparation of lac sulphuris variously exposed to the air. But, to come to more direct proofs, we are assured, in Short's first volume of his History of mineral waters, as to the cold waters of Harrigate, that both the mud and scum burnt with a blue flame, and smelt strong of sulphur; and that great quantities of yellow sublimed flowers of sulphur have

have been found under the basons of that well. And Dr. Peter Shaw affirms, that real brimstone, even found to be so by proper trials, hath been seen floating in the water like feathers, and separable by bare straining: and to all this agrees the observation I have frequently made on several of these cold waters, viz. certain light purple-coloured pellicles are frequently found floating in them, which, being dried, sparkle, flame, and stink, on the red-hot iron. But, to conclude, the operation and effects of several of these our cold waters, altogether similar to those of sulphur, abundantly confirm their impregnation with that mineral.

Thus it appears, that sulphur is not confined to the hot baths of Aix la Chapelle, and a few more abroad, but is found also in the cold waters of both England and Ireland; and as these have, of late years, been subjected to a minute examination, I shall subjoin a brief comparison between the one and the other from experiment and observation: thus,

1. Is the smell of the waters of Aix la Chapelle like that of the washings of a foul gun, or like that of the solution of sulphur in an alkaline lye? So is that of our cold waters called sulphureous.

2. Do Aix la Chapelle waters, taken from their source, turn silver of a gold colour, and blackish; and with its solution, and that of sugar of lead, exhibit a dark-coloured precipitation? So do our waters called sulphureous.

3. Does Aix la Chapelle water, on dropping distilled vinegar into it, exhibit a milkiness, analogous to lac sulphuris? So divers of the cold waters above-mentioned do also exhibit a white cloud with other acids.

4. Do

4. Do the waters of Aix la Chapelle and Borſel contain an alkaline ſalt? So do many of our cold waters above enumerated.

5. Is ſilver, borne in the pockets during a courſe of Aix la Chapelle waters, tarniſhed? The like effect hath been obſerved in ſeveral of our cold waters.

6. Do the waters of Aix la Chapelle yield flowers of ſulphur? and do ſome of the ſprings of Borſel precipitate a magiſtery of ſulphur? The like hath been obſerved in ſome of our cold waters above enumerated.

7. Are the baths of Aix la Chapelle of known efficacy in the cure of the itch, impetigo, vitiligo, and ulcers? So are the cold waters above enumerated, as appears from the ſignal ſucceſſes, which have attended their uſe, even in ſome of the moſt inveterate and rebellious diſorders of this kind.

Is there any raſhneſs then in concluding, that theſe our cold waters do alſo contain ſulphur ſubſtancially diſſolved in them, and differ from the hot ones of Aix la Chapelle and Borſel in nothing but heat, and the different proportions of impregnating minerals of the ſame quality?

Hence appears the great uſefulneſs of examining mineral waters in concert; for as various accidental circumſtances give occaſion to different appearances, the examination of a competent number and variety of them, helps to ſupply the defects in the hiſtories of ſome of them. And, if this were further proſecuted, I doubt not but ſulphur in ſubſtance might be obtained from ſeveral of them, as well as from Aix; which is therefore recommended to phyſicians and naturaliſts.

So much may suffice concerning such waters, wherein sulphur is the predominating ingredient; but there are others, wherein there are strong indications of its presence in a smaller proportion, and mixed with other minerals; and indeed, perhaps, few waters are without an admixture of it; for, beside several plain waters, especially such as contain the native alcali, and the purging waters, sea-water, the brine-springs, and the chalybeate waters, all which manifest a fetor by putrefaction, and some of them there-upon the like discolourations of metals as the sulphureous waters, the chalybeate waters, in particular, manifest a sulphureous admixture, by the cream, which they throw up to the surface, the various colours whereof, and its discolouring metals, are marks of sulphur.

There are, moreover, several other waters, even some of those, which otherwise make the nearest approach to pure element, having very little salts or earth, which I have mentioned in the beginning of this work, which also give strong suspicions of some degree of a sulphureous impregnation, by the purple and black sediments precipitated from them by solution of silver; which are eminently confirmed by a late examination of the celebrated Holy wells at Malvern, published in the 50th volume of the Transactions, and there extolled for many cures; which, altho' they do not yield quite a grain of solid contents from a pint upon evaporation, give three evidences of sulphur.

1. The purple powder precipitated from them by solution of silver. 2. In exhaling the water slowly in a silver vessel, the bottom of the vessel was tinged

of a pale yellow colour, as if it had been gilded. 3. When exhaled almost to a dryness, it emitted vapours of the smell of burning brimstone.

And lastly, besides all these, I have frequently remarked of several other waters, here and there mentioned in this work, that their sediments, obtained by evaporation, did manifest some pittance of this mineral, by the fetor they acquired on being rubbed with salt of Tartar; which, in the language of our author above-mentioned, being attracted by the acid, the phlogiston is let loose.

I have above recommended the farther investigation of sulphur as a desideratum in the history of mineral waters; and shall now beg leave to conclude this paper with the mention of two or three more articles, which greatly want further elucidation.

The first is alum, which, although hitherto found but in extremely few waters, and chiefly in those of Nevil Holt, Ballycastle acid water, and perhaps in the Hartfell water in Scotland, above-mentioned, according to some late experiments; yet the genuine crystals of alum have not as yet been satisfactorily demonstrated in any of these.

The second is the volatile mineral alkali, which the nitre of the ancients contained; and some of our mineral waters here and there give strong suspicions of, by the experiments with a mixture of quick-lime, and of the solution of mercury sublimata corrosive: but this matter greatly merits farther inquiry, and several difficulties attending it remain necessary to be explained.

Thirdly, the hint lately given in the 49th Vol. *Part. II. of the Transactions, of the efficacy of the
V O L. LI. O o Ca-

Carlsbadt water, as superior to lime-water, in dissolving stones out of the body, if confirmed by correspondent events in the internal use, would be a discovery of the greatest moment, and highly deserves to be prosecuted.

I shall only observe here, that the principal minerals impregnating it are a native alkaline salt, and a calcarious earth; and that the Aix la Chapelle waters are not without such a salt and earth also, and, which is of more moment, they are reported, by long taking, to render the urine alkaline, even as do the Carlsbadt waters; and we are also told, that calculi, macerated twenty-four hours in the water of Aix la Chapelle, have been reduced to a sand, or soft consistence: but how far this last, the Selters, the Bourne waters, or our Tilbury water, or others alike impregnated, may participate of a like virtue, must be determined by further observation and experience.

Dublin, the 24th 3d mo. 1759.

XXIX. *An Account of the Effects of a Storm of Thunder and Lightning at Rickmansworth, in Hertfordshire, on the 16th of July, 1759: In a Letter from Mrs. Anne Whitfeld. Communicated by Mr. John Van Rixtel, F. R. S.*

S I R,

Rickmersworth, O&A. 22. 1759.

Read Nov. 15,
1759.

MY son not being at home, I have taken upon me to comply with your request, in giving you an account of the damages

we sustained by the thunder and lightning on the 16th of July last. It was about a quarter after eight in the morning, when I had but just got out of the window where I had been sitting, and which is in the west side of the house, and was not got half-way cross the parlour, before a violent storm of thunder and lightning burst in at one of the sashes, and broke five large panes, and tore down the shutter, and shivered the window-seat, that the splinters passed by me to the farther end of the room; and one side of me was covered with the glass: one pane also was broke to pieces where I had been sitting. I had the mortification to stand so near the window, as to see all the tiles and a chimney shoot off from the house, as if shot out of a skuttle: the noise was so great, that at first I apprehended the drum of my ears broke, and it was some time before I was able hear at all, and some hours before I perfectly recovered my hearing. The sulphureous smell was so great, I cannot describe it, and the heat I felt on my cheek and head not to be conceived, without the same being felt. When it had ceased, I was going out at the door, and was met by my daughter, with her comb-tray under her arm, and split; who in a mournful tone said, Mamma, I am almost killed. On her telling me it had broke into her room, I immediately went up: I two or three times attempted going into her chamber before I could venture, and then not without covering my face with a thin apron I had on; the sulphur-smoke and dust being so great, that I was almost suffocated, besides it being so darkened by it that I could not see the window, which I felt out, and set open. When the dust and smoke had a little subsided, how deplo-

rable was the fight to me, and several of our neighbours! who by that time were come in. To give you some faint idea of it, tho' beyond my description, it had burst into one side of the room, where there had formerly been a window, now blocked up, but the iron-bars left standing, which were all forced down, and a very large hole made through the bricks, and the timber split, a splinter being carried cross the room, and stuck into the lead of a small window, that gave light to a passage; the bed driven at least two foot from its place; the rail the vallance were nailed to split in sunder; all the vallance were unnailed, and the rings torn off from the curtains, and some of the tester torn; the locks of a bureau and corner-cupboard forced open, with the bolts standing upright; some pictures were broken, and a little India cabinet broken to pieces, and almost every thing that was in the drawers of it: there was a small stand, with a wash-hands basin on it, and a decanter of water close to it; the basin was broken to pieces, and the decanter not hurt: the window was shattered to pieces, and the hangings of one side the room torn. My daughter was in the room at this time, and not hurt, any more than forced against the bed-post, suffocated with sulphur, and almost deaf as I was: it was thought her comb-tray preserved her arm, by keeping it hollow from her waist, as that was split, as if knocked in on one side with a hammer; and the door, very fortunately being open, gave some air, which otherwise could not be had, as there was no chimney in her room, and, as she said, assisted her in making her escape the sooner; for she only felt out the door; so that, had it been fastened, as it was two or three minutes before

this happened, she would have been suffocated before she could have got out : a memorable providence of the Almighty ! which she and I ought always to be thankful for. I then went into the next chamber on one side of hers, where the chimney had fallen from, that lay open to the air : a large stone round the chimney, instead of marble, hung dropping, which was forced to be taken down by two or three men, to prevent its breaking the marble hearth : the chimney-board and a brass hearth broken to pieces, as was the wainscot over the chimney : six panes of the sash-window were broken. I then returned from viewing the mischief done up-stairs, and went into the garden ; the door into it had a large piece of strong, sound oak split off, and carried away : a stone fixed into the gravel, to receive the water from the spouts, was thrown upon its edge, and smoked like a boiling pot, occasioned, I suppose, from being wet with the rain, that had fallen a little before the thunder-clap, that did this damage : about ten yards from this stone, the gravel was torn up as if with a plough-share. A little garden, that had two gates opposite each other ; the one, in the same west front as the house, was split to pieces ; and the leaden spout, that went down the side of the gate, was beat flat, and the ground round it thrown up : the other gate had its eell torn quite up : the lead over the door and windows of that side of the house rolled up like a sheet ; and, what is more amazing, the chamber, over the parlour that was so much damaged, hurt ; yet the two beyond it received so much damage, as I have before related ; and none of the rooms under the aforesaid chambers were in the least hurt. These,

Sir,

Sir, are the particulars of that day's misfortunes; but there were some more trifling damages, too much for this present time to admit me to recount: and this account you may depend on as authentic, as more than an hundred people can testify, that flocked in on that, and for several days after, to view. If this is any-wise satisfactory to you, it will give great pleasure to,

S I R,

Your humble servant,

Anne Whitfeld.

XXX. *An Account of some extraordinary Effects of Lightning, in a Letter to Dr. Gowin Knight: By Mr. William Mountaine, F. R. S.*

Dear Sir,

Read Nov. 22, 1759. **T**HE following account of the effects of lightning, in my neighbourhood, I have drawn up for your perusal; and, if it meets with your approbation, be pleased to communicate the same to the *Royal Society*, or dispose of it in any other manner, as you shall think proper.

During the morning of July the 16th last, was much thunder and lightning: about eight o'clock was heard an extraordinary loud crack, which seemed to me very near, as the large flash and sound were almost coincident. In a few minutes, there was an
alarm,

alarm, that an house was on fire in *Goat-street*. I readily suspected the cause, and soon after went to the place, in order to inquire into particulars, and was informed, that three houses were damaged in that place.

The first house I entered into was inhabited by Mr. William Loft, a custom-house waterman, which was almost untiled on the west side, and being of timber, was very much split and shattered: some of the weather-boards were thrown outwards to the bottom of the garden, to the distance of about thirty feet from the house, and the windows were forced inwards; but no damage was done to this house by fire.

It may not be amiss to mention, that several small pieces of glass, in the leaded windows, were impelled with such force, as to stick very fast in a door which was opposite, and in the hard plaistered partition; some of which I drew out of both, which, together with some bits of melted window-lead, you will find in the box N^o 1.

The second house, that I examined, was that of Mr. Arthur Tawke, a sail-maker, on the opposite side of the street, to the eastward. This was the house said to have been on fire; and here I found the following accidents and effects; viz. the window-shutters of the back parlour, on the east side, were shattered, and most of the bell-wire in this room melted; its track on the wainscot much scorched, but more so at the cranks: a hole was burnt in a copper-plate print, which hung under the wire; and, along the same side of the room, several rush-bottomed chairs were burnt in specks and holes of different

ferent forms and sizes : the floor had the like marks of burning, more especially under the course of the wire : on the projections of the wainscot, I found several granulations, and longer pieces of the wire, some of which were bedded in burnt cavities. A few of these, taken out by hand, I examined ; but could not find, that they were impregnated with any magnetic quality.

I afterwards employed magnets in search of the iron particles, which were found in the crevices of the wainscot, seams of the floor, and in the bottoms of the chairs, &c. which you will meet with in the box No 2.

The servant maid was standing in the door-way, between this room and the fore-parlour, when the stroke happened, and gives this very simple description of what she saw ; viz. “ that the appearance “ in the room was like a shower of fire.”—The dispersion and fall of the red-hot particles of the melted wire would make such a representation very natural.

In the fore-chamber, up one pair of stairs, which lies to the west, Mrs. Tawke was in bed, having lately laid-in : the flash alarmed her much ; but, having recovered from the fright, she perceived a sulphureous suffocating smell.—By her direction, a dark closet near the bed-side was examined, and found full of smoke and flames, which were soon extinguished. Hence arose the before-mentioned report of an house being on fire.

In this closet, I found the bell-wire, coming from the parlour below, to be intirely melted, or dispersed, but the effect ceased at the crank, which transmitted it to the chamber adjoining, where it remained intire.

A pair of striped cotton trowsers, at the distance of 4 or 5 feet from any part of the wire, were burnt almost to tinder, a piece of which is here preserved in the box N^o 5.

An old wig-box was burnt in part: a sheet was burnt through several folds, in large holes, and also a blanket; but the holes were smaller, tho' of different sizes: the velvet cape of a furtout coat, at a greater distance from the wire, was treated in like manner; tho' the burnt spots were in general smaller. Hence it seems, as if the particles of the fused wire did not *all* drop perpendicular; but that they were actuated by some impellent force, and that the smaller granulations were diffused to a greater distance; and hence arose that appearance of the shower of fire before-mentioned: and in this I am somewhat the more confirmed by some of the facts hereafter described. Even some of the larger pieces were thrown to a great distance; for here I found a wire mark burnt in the floor not less than six feet from any part of the suspended wire, and, on comparing a piece, which the maid picked up in the said place, with the said mark, it appeared to be the same, which produced that effect.—You will find it, being the largest in the box N^o 3.

A deal box, standing on a cloaths-chest under the the wire, was burnt in spots even more remarkably than the floor, according to the figures and forms of the several pieces, particles, or granulations. In this closet I also employed the magnets, and collected from the crevices, corners, &c. a quantity of iron particles, which are contained in the box N^o 3.

The third house, inhabited by Mr. Robert Harris senior, corn-factor, lies at the north-east corner of the same street, at the distance of about 40 yards. Here I found some damage done to the glasses and China ware in a closet contiguous to one of the bell-wires in a ground room: most of the wire in this room was melted; and a piece of the deal moulding, nine inches long, covering the wire, and adjoining to the brass thumb-piece, very near the said closet, was splintered off, and struck the servant maid in the face, as she was entering the room, at about 14 feet distance.

In some of the rooms in the second story, the wires were in part melted. In one room of the third story the wire was intirely dissipated; the wall scorched; the whole plaistering over the door, adjoining to the bell, driven out in a body; the floor burnt; and the sheets and quilt of a bed, near the bell-wire, scorched and fire-pitted in like manner as at Mr. Tawke's; only the effects of the ignited particles were not so general through this house, nor was any thing here absolutely set on fire.

I was afterwards informed, that tho' all the wires were not destroyed, yet they had been obliged to renew the whole; for, when they came to be examined, they were found so unpliable and brittle, as to be rendered quite useless.

On the 28th of July I went again to Mr. Tawke's, who, being then at home, conducted me to a garret, which lies partly over the before-mentioned dark closet, from whence a bell-wire was directed to this room, by me unobserved before; nor did the family very soon discover, that this wire was intirely melted, and the partition greatly scorched.

This

This room contained a large heap of oats, and another of beans, a foot-path, as it were, being reserved next the partition, where the ascending wire ran, which was covered with much dust and straggling grains of the corn.—Here again I employed the magnets, Mr. Tawke assisting, and collected a considerable quantity of melted particles, not only from the floor, joints and projections of the skirting-board, but from the holes and chafms in the broken plaistering, where-ever we could introduce the magnets, and that at the distance of 4 or 5 feet from the wire-place. These you have in the box N° 4.

Hence I suspected, that *granulæ* might be found among the oats: we probed, but to no purpose; the heap was too large, and had been moved, by fetching away what was wanted from time to time. However, we carried our inquiry to the bare floor, quite across the heap, at 10 feet distance, and, in the joints or seams, found of very fine particles a sufficient quantity to prove, that they were violently diffused to a great distance.

From these houses the lightning seems to have tended towards the N. E. for in that direction, at the distance of about 200 yards, and not so far from my own house, Captain William Provost was struck thereby, standing in his own entry, and rendered almost senseless and speechless for some hours, and, for several days, was much afflicted with a stupor, giddiness, and vomiting, and retained a constant and strong taste of sulphur in his mouth and throat. His child had hold of his cloaths, and his wife was near him; but the stroke appears to have been above the child, as it seems to have struck him about the head. No other damage was done here.

Mrs. Provost says, that the flash or fire seemed to be as large as a small pewter plate, and passed clear through the entry (the doors being open) directed to the northward.

On the opposite side of the street, and somewhat oblique towards the north-east, at the distance of about 20 yards, Mr. Ambrose Lyon, sail-maker, had, at the same time, about six dozen of bottles of Port wine broken to pieces.

The front of Mr. Lyon's house has nearly a south-west aspect: among other conveniences under-ground is a substantial arched brick vault, and capacious for a private house; it is quite close, having no light or opening into it, but at the door, which faces the south, and is always kept locked.—On the western side of this vault were repositied several casks of wine, most of them iron-bound; also several dozens of rum behind them upon the floor. On the northern end was a circular tub or cooler, iron-bound with three hoops, containing Port wine in bottles. About ten feet from hence, on the eastern side, nearer the door, and directly opposite thereto, stood by itself another large circular cooler, 32 inches diameter, and 12 inches deep, bound also with three iron hoops, containing about six dozen of Portugal wine, called *Barabarba*, the remainder of a larger quantity, which he had kept in cask for more than two years before it was bottled, in which last state it had been for more than three months, and all proper care taken of it; for he is very curious in these things. These bottles were inclined upon their sides, for their better preservation, as were all the others containing wine; and this tub was fixed upon skids (pieces of timber) about six inches thick.

In half an hour, or less, after the great thunder-clap, having occasion to go into the vault, upon his entrance, he immediately discovered the noise of a running drip, and liquor flowing about the floor, which, upon further inspection, he found proceeded from the last-mentioned cooler, the bottles therein contained being broken to pieces, as if done by a mallet, not so much as one remaining intire of the whole quantity; and the cooler being not very tight, the wine was running out from thence; but, by a quick application of such vessels as were at hand, the remainder was preserved, and, after clearing it from the glass, was put into a small cask to recover, adding thereto about a one-third part of Port wine to fill up the vessel, some of which I have tasted; and a bottle, drawn from the said cask, I herewith send you.

Mr. Lyon did not observe, at the same time, any kind of alteration in the said wine made by fermentation, or otherwise; only that it was vapid or flat: one bottle of the said wine was afterwards found among the bottles of Port in the first-mentioned cooler, which was not affected, but was fine, and in perfection; nor was any other damage done in the vault, that he could discover upon the strictest inquiry.

Captain John Dickinson, a gentleman in the neighbourhood, shared half the cask of the said wine, which had a similar management, was bottled off at the same time, and had a less cool and friendly vault; yet not one bottle of this has hitherto been in anywise damaged.

I have

I have been more particular in the examination of some of the foregoing facts, as they seem to contradict an opinion generally received, that the solution of metals by lightning is effected by a kind of cold fusion; for it appears very evident, that the melted iron-wire, in the several preceding cases, had all the marks of heat and ignition, that usually attend the fusion of that metal, when brought about by common fire. I am,

Dear Sir,

Your most obedient servant,

Gainsford-street, Southwark,
Septem. 28th, 1759.

Wm. Mountaine.

*Some Remarks on the preceding Letter, by
Gowin Knight, M.B.F.R.S. and Principal
Librarian of the British Museum.*

Read Nov. 22, 1759. **T**HE facts, contained in Mr. Mountaine's letter, are an evident proof, that the fusion of metals by lightning is, sometimes at least, attended with heat and ignition, as in the case of common fusion. And, since the reading of those facts, I have been more and more induced to suspect, that the received opinion of a cold fusion is a vulgar error, tho' too generally adopted, and of very long standing. From some of the circumstances attending these facts, compared with what is to be found in authors relating to the same subject, I think it possible both to shew, whence this opinion first took its rise, and how it became so general; and at
the

the same time to prove, that there is no clear evidence for the truth of it from any relations hitherto published.

The instances, that are most generally given of cold fusion, are two; that of a sword being melted in its scabbard, and that of money being melted in a bag, both the scabbard and bag remaining unhurt. A great number of authors have mentioned both the facts, without giving their own testimony, or that of any one else, for the truth of them, or describing any of the other concomitant circumstances. However, it seems possible, that lightning might produce effects sufficiently similar to these, to give rise to such reports, without our being obliged to have recourse to a cold fusion to account for them.

If at any time the edge or external surface of a sword had been melted, whilst the main part of the blade remained intire, it would have afforded sufficient grounds to assert, in general terms, that the sword was melted, and yet the scabbard might have remained unhurt; because either the edge or surface of a sword might be instantly melted by lightning, and cooled so suddenly, as to make no impression of burning on the scabbard. Metals, as well as other bodies, will both heat and cool sooner, in proportion as they are thin and slender. Very small wire will instantly become red-hot, and even melt, and run into a round globule, in the flame of a common candle; and it is no sooner removed out of the flame, but it is as instantly cool. The edge of a sword therefore, or even its surface, might be instantly melted by lightning, and being in contact, or rather still united to the rest of the blade, which might be still cool,

cool, it would part with its heat too suddenly to produce any appearance of burning.

I was confirmed in this reasoning, by examining the fragments and melted particles of wire sent me by Mr. Mountaine. Amongst them there appeared to be globules of various sizes, which had undergone very different degrees of fusion: the largest of these had not been fluid enough to put on a spherical figure; but they approached nearer thereto, in proportion as they were smaller: so that in the smallest *granulæ* the fusion was most perfect, the globules being very round and smooth. Their sizes continued diminishing, till they became invisible to the naked eye; and some of them, when viewed with a microscope, required a third or fourth magnifier to see them distinctly.

Some of the bits of wire were rough and scaly, like burnt iron, and were swelled in those places where they were beginning to melt: others continued strait, and of an equable thickness; but their outward surface seemed to have undergone a perfect fusion, so that there were two or more pieces adhering together, as if joined by a thin solder.

In Mr. Pitfold's account of the effects of lightning at Darking in Surry, published in the Philosophical Transactions *, mention is made of a similar fact. He says, "some small tacks were soldered together, six, seven, eight, or ten in a clump, as if they had had scalding metal run over them."

It is easy to conceive, how the heat of this superficial fusion might be so suddenly diffused throughout

* Philof. Transf. Vol. XLIX. p. 311.

the metal it surrounded, which the lightning might not have heated, as instantly to have reduced the whole to too cool a state, for any other contiguous body to be burnt thereby.

In like manner, a stream of lightning passing thro' a bag of money, might fuse the surfaces of such pieces as lay in its way, and solder a number of them together; and yet the bag remain unhurt.

An accident or two of this kind may have come, by tradition, to the knowledge of some of the first collectors of marvellous facts, and from them be transcribed by others, perhaps with additions and improvements. Thus, according to Pliny*, both gold, silver, and brass, have been melted in bags sealed up, which were not in the least burnt, nor was the wax of the seals melted: whereas Seneca† speaks only of silver being melted in the pocket or purse, which remained whole and unhurt. Later writers seem to have copied from one of these for the most part, without mentioning their authority.

In the Philosophical Transactions are two or three relations, which seem, at first, to favour a cold fusion; but, when duly considered, prove nothing conclusively. The first is in a paper concerning the effects of lightning at Colchester, on July 16, 1708; which concludes with observing, that, “during the same storm, four persons were killed in a boat, that was going from Harwich to Ipswich; and that, in one of their pockets, a watch and chain was melted all on a lump.” In another account, given by Or-

* Plin. Nat. Hist. lib. ii. c. 51.

† Seneca, Nat. Quæst. l. ii. c. 32.

lando Bridgman, Esq; describing the same storm, it is said, that "the chain of the watch was melted, " and that no harm or burn could be perceived on " his breeches or cloaths." Now, if both the watch and chain were melted all on a lump, and the pocket unburnt, as might be concluded from both these accounts laid together, it would be a strong argument in favour of cold fusion: but there is great reason to suspect the truth of the first-mentioned relation; because the author of it writes only from hearsay, being himself at Colchester, at several miles distance from where the thing happened. Whereas Mr. Bridgman was upon the spot, and examined one of the dead bodies himself very minutely; and tho' he does not say, that he saw the body of him, whose watch-chain was melted, yet he gives a very circumstantial account of it; and, if the watch had been melted, as well as the chain, he could not have omitted that particular. It is therefore probable, that the chain only was melted, and that, hanging out of the pocket, it had left no marks of burning on the breeches.

We have, in our Transactions, another account of the effects of lightning by Dr. Cookson, of Wakefield †, who relates, "That the lightning fell on a " box of knives and forks, and melted a great many " of them, the sheaths being untouched." But the doctor, in another account, which is fuller and more exact, says, "the lightning dispersed a great " many dozen of knives and forks, which were put up " in a box, all over the room. Upon gathering them

* Philos. Transf. Abr. Vol. V. p. 154.

† Ibid. Vol. VIII. p. 504.

“ up, some of them were melted ; others snapped in
 “ funder ; others had their hafts burnt ; others their
 “ sheaths either singed or burnt ; others not.” From
 all which circumstances, duly confidered, I think no-
 thing certain in favour of cold fusion can be fairly
 drawn.

XXXI. *An Account of a Meteor feen at
 Shefford, in Berkshire, on Saturday, Octo-
 ber 20th, 1759 ; with some Observations
 on the Weather of the preceding Winter :
 In a Letter to Thomas Birch, D. D. Sec.
 R. S. from Richard Forfter, M. A. Rec-
 tor of Shefford.*

Reverend Sir, Shefford, Octob. 31, 1759.

Read Nov. 8, 1759. ON Saturday the 20th instant, about
 Six in the evening, a ball of fire
 fell nearly east from this place. I did not see it my-
 self. My servant (who is a very sober, honest fel-
 low) says it was nearly of the same size with the
 moon, and full as bright as she ever shines : its mo-
 tion was very swift, and, as far as he could judge
 (for it was out in a moment) quite downright, i. e.
 perpendicular to the horizon.

And now my hand is in, I cannot forbear acquaint-
 ing you with an observation I have made, which bids
 fair to overset a maxim pretty strongly established in
 the world, as not being only believed and depended
 on by the vulgar and middling people, but mentioned

as such, I think, by several authors. In short, the maxim is this; viz. that a plentiful year of mast is an infallible prognostic of an hard or severe winter. Now, it happened last year, that provisions of this sort were as plentiful as ever was known; the trees and hedges being loaded in such a manner, as to bend and break under the pressure of their own weight: and yet the winter was the mildest, perhaps, that ever happened in this country: and accordingly not one quarter of nature's store was consumed. We had no ice, but once, and that not the thickness of an half crown, which did not continue 24 hours. I see by Cuff's tables, published in a monthly paper, that, in London, the thermometer was never below 32; and so low as this but twice, and then only by starts. I had ranunculus's in full bloom from the middle of December to the middle of February, and they not sheltered, but by a wall north, 25° east. In the middle of January, I had self-sowed marigolds and violets in bloom. Jan. 15, the bees roared, and were as busy as they are in the height of the working season: and Jan. 18, the birds sung as chearfully as they generally do in May.

It seems probable to me, that the great abundance of berries and wild fruits (by which I mean mast) is intirely owing to a very backward spring; for, when the blossoms do not open till pretty late in May, they are secure from those inclement blasts, which, when they unfold themselves sooner, do pinch and blight the greatest part of them. I am,

Reverend Sir,
 Your affectionate brother,
 and most obliged humble servant,
 Richard Forster.
 XXXII. *An*

XXXII. *An Account of the same Meteor, seen at Bath: In a Letter to Tho. Birch, D. D. Sec. R. S. from Mr. Josiah Colebrooke, F. R. S.*

Dear Sir,

Read Nov. 15, 1759. **I**N compliance with the president's desire, the following is the account of the meteor seen at Bath.

On Saturday the 20th of October, between five and six in the afternoon, as I walked over the north parade, a ball of fire, of the bigness of a tennis ball, of a very bright colour, with a train of four or five feet in length, darted from the north-west, and, describing the arch of a great circle on my left hand, sunk behind the hills to the south-east: just before it sunk, several large sparks of bright blue fire issued from it; but it did not seem to burst: it was not more than two seconds in its passage, and I could compare it to nothing, but the most glorious sky rocket I had ever seen.

Mr. Peers, a gentleman of London, was at Bath at the same time, and being in a room fronting the east, that looked over the meadow between Bath and Bathwick, he told me, with some surprize, the next day, that he saw the largest star he had ever seen, fall into the meadow; and, what was most particular, that it fell perpendicularly; whereas all he had ever seen before shot obliquely in the sky.

This must have been a spark from this meteor, as the time he saw the star agreed with the time I saw the ball of fire.

I do

I do not remember, that I heard any noise or whizzing in the air as it passed; nor did any sulphureous smell attend it, that I could perceive.

I looked on my watch immediately after it was gone by, and found by that, it was just twenty minutes past five; but, as I could not be sure that went right, I chose to mention the time in more general terms.

It was seen by vast numbers of people, and much talked of the next day.

I am, dear Sir, with much esteem,

Your very humble servant,

Budge Row, Nov. 14. 1759.

Josiah Colebrooke.

XXXIII. *An Account of the Meteor Seen at Chigwell Row, in Essex, on the 20th of October 1759: In a Letter to the Rev. Dr. Birch, Secretary of the Royal Society, from Mr. William Dutton, Watchmaker in Fleet-street.*

SIR,

Fleet-street, Dec. 1, 1759.

Read Dec 6, 1759. **I**N compliance with Dr. Pringle's request, I send you the following account of the meteor, which I saw some time ago, viz. on Saturday, Octob. 20th, about a quarter before six in the evening, whilst there was still some day-light, tho' several stars had begun to appear.

At that time I happened to be walking with my face eastward, with a companion, at Chigwell Row,

in Essex (which lies about 12 miles east of London, and upon a pretty high hill), when we observed a meteor bearing northward of the east, in appearance not high up in the air, tho' with a considerable angle of elevation, perhaps of about 70 or 80 degrees, with its declination from the zenith eastwards. It moved with great velocity, in a direction from north to south, and seemingly in a curve line downwards; but vanished at the height of 4 or 5 degrees above the horizon, then bearing nearly south of us. It was of a round form, about the size of the planet Venus, when seen at the largest, of a light bluish cast, but very bright. At its vanishing, several particles, still brighter than itself, and somewhat like the stars, that are seen upon the breaking of a rocket, seemed to issue out of it. We perceived a faint light to follow it, like the tail of a comet, and about two feet in length. The whole time of the appearance did not exceed 3 or 4 seconds. This is the best account I can give of what I saw; and my companion, to whom I have shewn this letter, agrees with me in all these circumstances. I am,

S I R,

Your very humble servant,

Wm. Dutton.

XXXIV. *An Account of Two Stones of remarkable Shapes and Sizes, which, for the Space of Six Years, were firmly lodged in the Urethra of a young Man, and at length successfully cut out from thence. Addressed to the Royal Society, on Thursday December 13, 1759, at which Meeting the Stones themselves, and a Drawing of the Stones, were presented to the Fellows of the Society, by Joseph Warner, F. R. S. and Surgeon to Guy's Hospital.*

Read Dec. 13, 1759. **T**HE formation and confinement of stony concretions in the different parts of the organs of urine, to wit, in the kidneys, ureters, bladder, and urethra, are diseases, which are frequently observed to happen to both sexes of all ages and constitutions: for this reason I am apprised, that there are few instances can be given of such peculiarities attending these cases, as may reasonably be esteemed worthy the attention of the curious. But when matters of fact, however common in themselves, are so circumstanced, as to assume extraordinary appearances, the uncommon phenomena accompanying such facts, when capable of being pointed out, will, I believe, be always considered by the Royal Society, as a sufficient apology for the freedom of the communication. Upon this presumption, I have taken the liberty of laying before the Fellows of
this

this Society the following short account, and, at the same time, of submitting to their inspection as much of the case, as the nature of the affair could admit of being exhibited.

C A S E.

Thomas Bingham, a very healthy young man, 20 years old, came from Yarmouth to London, in September 1759, and put himself under my care, to be cured of a swelling; which he had in his urethra. Upon questioning the patient, I was informed by him, that he had little or no pain; that he had never perceived the least difficulty in voiding his urine, nor had he ever had the least involuntary efflux of it: he had not at any time suffered in the least but from the bulk and weight of something that grew in his urinary passage (urethra), which, upon exercise of late, proved troublesome to him. Upon inspection, I discovered a considerable prominence betwixt the testicles and anus. Upon feeling the part with my fingers, there appeared a very evident hardness and tumor.

By introducing a large, smooth, and ductile, probe into the urethra, it was evident there was a stone or stones lodged in that passage. I advised the cutting the tumor out, which was complied with; and in the following manner I proceeded to the performance of the operation.

The patient being supinely placed upon a steady table, of a convenient height, covered with a double blanket, and a pillow put under his shoulders, I caused his hands and feet to be tied together; and, by proper assistants, he was held in the same position as

is done in the operation of cutting for the stone in the urinary bladder. I then proceeded to divide the urethra longitudinally by incision. The extent of the incision was from one end of the swelling to the other : the length and size of the wound enabled me to take away the stones without any violence or difficulty.

After the stones were removed, I brought the lips of the wound together, and, with the twisted suture, I retained them in that situation. By this method, and by occasionally passing a bougie of a proper size into the urethra, beyond the farther extent of the incision, the patient went happily on, till the cure of the wound was completed, which was effected in about three weeks ; and there afterwards remained no inconvenience at all to the patient in voiding or retaining his urine.

N. B. As the identical calculi are presented to the Fellows of the Royal Society for their inspection, so that the exact sizes, shapes, and external formations, of these stones may be seen ; I think it quite unnecessary to give a written account of these particulars. However, as it may probably give some satisfaction to the curious to be informed of the specific gravities of these stones, I have subjoined these particulars to this memoir, and have likewise, for the same reason, caused an engraving of these stones to be made, and added to this paper.

Fig. 1.

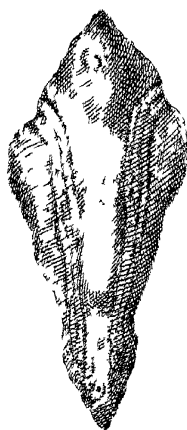


Fig. 2.



	Weight in air.	in water.	Spec. gravity.
Large stone	308 5	92 85	1.431
Small stone	42 35	10 4	1.326
Both stones	350 8	102 9	1.415

Quære. Is it not probable, that the generation of these stones might originally have begun in the urethra, as the patient did not ever remember to have had the least complaint in his loins, or in any part of his bladder? Or is it more reasonable to suppose, that the stones were first of all formed in the kidney, or urinary bladder, from thence conveyed with the stream of urine, when very small, and lodged in the urethra, till they had arrived to these sizes?

Either of these suppositions to me appears reasonable.

However, as hypotheses of this kind are with difficulty ascertained, and as a discovery of the fact, could it be determinately made, would prove of very little consequence to the improvement of the art of surgery, I think it quite unnecessary to dwell upon a part of this subject, in which the benefit of mankind appears to be so little concerned.

Plate VIII. Fig. 1. represents the upper parts of the two stones, as they were found lying loosely together in the urethra. The longest end, that is, the smallest of these two stones, was situated nearest to the neck of the bladder, or origin of the urethra. It may be observed, that, on the superior part of these stones, there are two long grooves or chanel, which were gradually formed by the streams of urine, that were occasionally discharged from the bladder.

It may farther be observed, that the whole surfaces of these two stones, except the parts of them where they lay in contact with each other, or where they continually pressed upon the lower surface of the urethra, or bed, which they had formed for themselves in this chanel, are rough, and have several inequalities, or eminences arising from them.

Fig. 2. represents the inferior smooth parts of the two stones, as they appeared when separated from each other, as well as the smooth or polished surfaces of the ends of these stones, which lay in contact with each other, and upon motion rubbed against each other,

XXXV. *Experiments on the Tourmalin: by Mr. Benjamin Wilson, F. R. S. In a letter to Dr. William Heberden, F. R. S.*

S I R,

Read Dec. 6. 1759. **I** Have the pleasure to communicate to you some experiments made upon the *Tourmalin*, or *Ashstone*, which you were so kind as to procure me, together with some others, respecting the vitreous and resinous electricities, as they are called, and the observations I have made thereupon.

The more I am acquainted with electricity, the more I admire a wonderful simplicity which seems to prevail in nature, at least in this part which abounds with phænomena of a very curious kind; whereof many that have passed under my examination of late are so extremely nice, that I avoid venturing to relate them, because I would not willingly subject myself to the censure of incautious observers.

I cannot enter into this particular subject, without first settling a dispute amongst electricians, which subsists at this day, concerning the two electricities; because some consequences, drawn from the several experiments I have to produce, greatly depend upon it.

Polished glass, upon being rubbed properly, has been supposed to *give* an electricity to bodies, and those bodies that receive it from the glass, are said to be electrified *plus*. Whereas wax, amber, &c. upon being rubbed in the same manner, have on the contrary been supposed to *receive* an electricity from bodies, and those bodies which part with it are said to be electrified *minus*. But no experiment, that I know of, has yet appeared to determine which of these electricities does really electrify *plus*, and which of them does really electrify *minus*; though it happens that the fact turns out just as they have all along supposed.

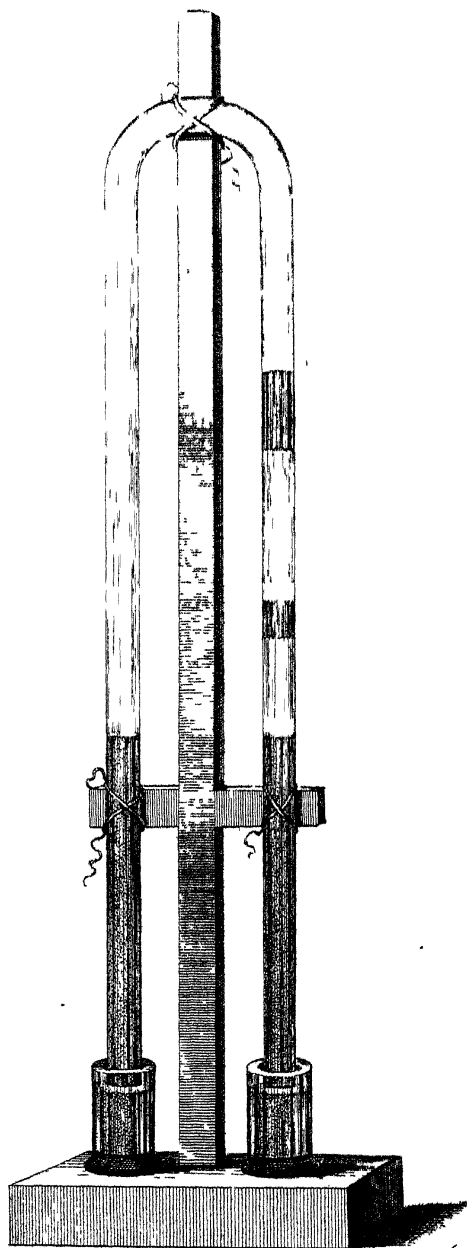
In a second treatise upon the subject of electricity, that I published in the year 1748, several experiments were produced, to shew that all bodies are surrounded with a *medium*, which is *of an exceeding elastic nature, and extends but to a very small distance from the body* when it is not disturbed by heat, or other causes. Since that, other experiments of the like kind have been published in a work, wherein my late worthy friend *Dr. Hoadly* was concerned with me. Among the proofs therein given, is a curious one, which I observed in the *Torricellian vacuum*, where the appearance was remarkably sensible. And what is more singular, that same appearance not only proves the existence of a medium, at or on the surface of bodies, but at the same time determines which
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of the electricities is truly *plus*, and which of them is *minus*.

You must remember, *Lord Charles Cavendish* first observed the luminous appearance in one continued stream throughout the whole *vacuum* of an exhausted tube. It is a fine experiment, and affords more information than I at first imagined, therefore I shall beg leave to recite it from the *Philosophical Transactions* *, before I give you any farther particulars about it.

“ This apparatus consisted of a cylindrical glass
 “ tube of about three tenths of an inch in diameter,
 “ and of seven feet and half in length, bent some-
 “ what like a parabola, in such a manner, that thirty
 “ inches of each of its extremities were nearly
 “ straight, and parallel to each other, from which
 “ an arch sprung, which was likewise of thirty in-
 “ ches. This tube was carefully filled with mer-
 “ cury; and each of its extremities being put into its
 “ basin of mercury, so much of the mercury ran
 “ out, until, as in common barometrical tubes, it
 “ was in equilibrio with the atmosphere. Each of
 “ the basins containing the mercury was of wood,
 “ and was supported by a cylindrical glass of about
 “ four inches in diameter, and six inches in length;
 “ and these glasses were fastened to the bottom of a
 “ square wooden frame, so contrived, as that to its top
 “ was suspended with silk lines the tube filled with
 “ mercury before mentioned; so that the whole of
 “ this apparatus without inconvenience might be

* Vol. xlvii.



“ moved together. The *Torricellian vacuum* then
 “ occupied a space of about thirty inches. In make-
 “ ing the experiment, when the room was darken-
 “ ed, a wire from the prime conductor of the com-
 “ mon electrical machine communicated with one
 “ of the basons of mercury, and any non-electric
 “ touching the other bason, while the machine was
 “ in motion, *the electricity pervaded the vacuum in*
 “ *a continued arch of lambent flame, and as far as*
 “ *the eye could follow it, without the least divergen-*
 “ *cy.*”

I imagine *Dr. Watson*, who has described this experiment, did not, when he made the experiment, attend to a singular appearance of light upon one of the surfaces of the quicksilver, because he has taken no notice of it. However, I had a mind to make the experiment myself, and try whether I could not manage the quicksilver, so as to have more than two visible surfaces, in order that I might have that remarkable appearance repeated.

To do this, I let a very small quantity of air into the tube, by which means four columns of quicksilver were obtained, reckoning great and small together; and six visible surfaces, three of which I called upper, and three under surfaces. But the figure in the plate N^o. IX. will give you a better idea of the instrument, than I am able to do by writing.

When the column of quicksilver on the left hand was electrified, and the other on the right communicated with the earth, the stream of light was visible in a darkened chamber, and the general appearance, all the way through the *vacuum*, was a light of a seeming uniform density, excepting at the upper surfaces, for above one tenth of an inch, reckoning from

from each surface; and there the light was always considerably brighter; insomuch that a gentleman, then present, who was unacquainted with experiments of this kind, desired to know the reason of the three *knobs* of light appearing on the tops of the quicksilver. I mention this circumstance, because they were remarkably conspicuous; whereas the three under surfaces exhibited no such appearance, the light being rather less bright than even in the general appearance of the whole illuminated *vacuum*.

An electric current, setting in from the glass of the electrifying machine, and passing along the tube through the quicksilver and *vacuum*, and so to the earth, must have caused those bright *knobs* by means of the resistance the fluid met with at the upper surface of the quicksilver in endeavouring to enter it; because the appearances were alike on the three upper surfaces, and nothing of the same kind was seen at the under surfaces. Glass therefore electrifies bodies *plus*, or, in other words, gives bodies a quantity of electric fluid more than they have naturally.

I am now to acquaint you with the appearances that the *minus* electricity occasioned.

Instead of the glass cylinder to electrify with, I put a resinous one, preserving the communication, and every thing else, as in the former experiment. In these circumstances, the general appearance of the light in the *vacuum* was the same; but in this experiment the *knobs* of light were on the under surfaces, and not upon the upper.

From the knobs of light being at the under surfaces, and not upon the upper, I concluded that the flow of the fluid was the contrary way to that caused
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by glass; for in this case it appeared to come from the earth, then into the tube, and so on to the resinous cylinder. Those bodies therefore which have no communication with the earth, and from which the resinous cylinder is supplied, must be electrified *minus* *.

These luminous *knobs* (were there no other proofs) are a strong confirmation of the existence of a *medium* at or on the surfaces of bodies, which, to a limited degree, resists or hinders the entrance and exit of the electric fluid. But we have some other experiments to illustrate the same, and the *Tourmalin* does assist us not a little.

There is one thing more, which I am obliged to take notice of, concerning the impermeability of glass by electricity. Our friend, *Dr. Franklin*, seems to have found a great part of his system on this opinion.

“ Glass, says he, has within its substance always
 “ the same quantity of electrical fire, and that a very
 “ great quantity in proportion to the mass. This
 “ quantity, proportioned to the glass, it strongly and
 “ obstinately retains, *and will have neither more*
 “ *nor less*, though it will suffer a change to be made
 “ in its parts and situation; *i. e.* we may take away
 “ part of it from one of its sides, provided we throw
 “ an equal quantity into the other.”

This doctrine I could never agree to, so far as it relates to the impermeability of glass, as appears by my letters in answer to the Abbé *Nollet* and *M. le Roy*, on their requesting to know my sentiments about it,

* The same cylinder made rough with emmery, and rubbed with flannel at one time, and with leather at another, will afford these different appearances, the first causing the *minus*, and the last the *plus* appearance.

in the year 1756, the former of whom it is well known could never come into it*. And I am still the less inclined to alter my opinion of this matter, from an acquaintance with the properties of the *Tourmalin*, which I think has furnished me with sufficient grounds for advancing that *Dr. Franklin* is mistaken. I have wondered that *Æpinus* did not take notice of some of the experiments which electrify glass either *plus* or *minus*, because the *Tourmalin* affordeth leading experiments towards it; and can ascribe it to no other cause than a favourable opinion he was willing to entertain of *Dr. Franklin's* hypothesis.

To prove that glass is permeable by electricity, I took a very large pane thereof (I chose it large, that no objection might be made to the experiment) and then warmed it a little, so that it was free from moisture. After that, I held it upright by one edge, whilst the opposite edge rested upon wax, and rubbed the middle part of the surface with my finger. After this, I found that both sides were electrified *plus*. Upon repeating the experiment many times, with different glass and different degrees of rubbing, still both sides were in every experiment electrified *plus*. Now they could not both be electrified *plus* by this treatment, unless some part of the fluid had really passed through the glass; because the virtue could not get round to the other side over so great a surface: besides, the extremities of the glass were not electrified at all. As to electrifying glass *minus*, I find it may be done three different ways. But these and other experiments relating to the permeability of glass, I shall

* Vid. lettres sur l'électricité par M. l'Abbé Nollet, lett. iv.

forbear mentioning, till I have brought you better acquainted with the *Tourmalin*.

The first account I met with of the *Tourmalin*, and the remarkable properties belonging to it, was from a *memoir in the Berlin acts*, printed in the year 1758; wherein it appears, that *F. V. T. Æpinus*, professor of natural philosophy, made several very curious and judicious experiments upon it; the most material of which, prove a *plus* electricity on one side thereof, and, at the same time, a *minus* electricity on the other side; provided the *Tourmalin* is moderately warmed, and even by hot water. These appearances are the more extraordinary, as the like means employed in the same manner upon diamonds, glass, and all other electric bodies hitherto tried, produce no such appearances.

The *Duke de Noya*, who visited this kingdom in 1758, wrote a small treatise on the subject, and published it at Paris on his way to Italy: in this work he mentions *Æpinus*'s experiments, but does not admit of a *plus* and *minus* electricity belonging to the *Tourmalin* when heated. On the contrary, he says that the two sides are electrified *plus*, but one of them is more so than the other; and that it is the difference between those degrees which has led *Æpinus* into the mistake.

I remember to have repeated most of the experiments mentioned in the *Berlin memoir*, soon after it appeared in England, with the *Tourmalin* belonging to our friend *Dr. Sharp*, which you fortunately recollected to have seen in his possession many years ago at Cambridge; for it was the only one known of here at that time: and though but a small one, compared

with *Æpinus's*, yet it was large enough to satisfy me that his opinion was well founded. Besides, the trials you made with the same stone before I had it, were a farther confirmation of the truth. However, the following experiments, which I made to procure more *data* towards attaining some simple explanation of these curious phænomena, will sufficiently prove, that one side of the stone is really electrified *plus*, and the other side *minus*. And had the *Duke de Noya* made farther experiments, and pursued the same method I have done, I think he would have been of the same opinion.

The largest *Tourmalin* I had from you, and with which I made the following experiments, weighs above 120 grains. It is of an oval form, and polished: the greatest diameter measures an inch and a quarter, and the least one inch. One side is *plain*, the other is *convex*, but cut into several small planes or *facettes*, something like a rose diamond; the thickest part of which is near one third of an inch. This shape does not seem to me to be the most convenient for making experiments, but I would not alter it, lest the stone should break; for there are several cracks in it; and I fear it will be difficult to meet with another of the same size.

To make experiments with the *Tourmalin* requires the greatest attention, as the appearances sometimes are scarcely sensible, insomuch that I have been obliged to employ the tenderest kind of apparatus, and even interpose a sort of skreen to prevent my breath, or other like motions of the air, from disturbing the experiment.

My apparatus for making many of these experiments, consists of two very small balls made of the
pith

pith of elder, and suspended by two linnen threads of the finest kind: the ends of these threads I fasten to a slip of wood about three inches long, and half an inch broad: then upon a stick of sealing wax nine inches long, fixed upright on a table, or any other convenient place, I fasten the slip of wood, from one end of which the threads &c. hang down five inches, so that the pith balls are about four inches from the table. These balls are always supposed to be electrified *plus*, except where the contrary is mentioned: but with no greater power, than to make them recede from each other about one inch in every experiment.

I prefer the wax stand to a glass one, as the latter when cold acquires moisture very soon, and therefore becomes a conductor: whereas wax, when it is once in good order, will continue a non-conductor for a long time.

Before I set down my experiments, I must mention three truths that are commonly known, and which, for the present, I shall call

GENERAL LAWS.

Two bodies equally electrified *plus* recede from each other, or are repelled.

Two bodies electrified *minus* recede also from each other, or are repelled.

One body electrified *plus* and another body electrified *minus*, to an equal degree, accede, or move towards each other, or, as it is generally expressed, are attracted.

EXPERIMENTS upon the TOURMALIN.

Exp. 1. One edge of the *Tourmalin* being properly fastened to a long stick of sealing wax, I dipped the

stone into boiling water, and continued it there near one minute. On taking it out, and presenting the *convex* side thereof near the pith balls, they immediately *receded* from it, but not very strongly.

On turning the *plain* side towards the balls, it caused them to *accede* to it, but rather at a greater distance in this case, than they *receded* from it in the last. When the *stone* was colder, these appearances were considerably stronger; but, on cooling still more, they were less and less.

Exp. 2. I repeated this last experiment, with the difference only of having the balls electrified *minus* instead of *plus* *. And in this case (as might be expected) the effects were reversed; the balls *acceding* towards the *convex* side, and *receding* from the *plain* side.

These appearances in the two experiments agreeing with one another, sufficiently prove, that *when the stone has been heated by water, one side is electrified minus, and the other side is electrified plus*; which is the first law laid down by *Æpinus*. This state has not improperly been called its *natural state*; because the *heat*, which disposed the *Tourmalin* to afford these appearances, was *uniform* in every part of its surface, and the *water* itself an *uniform* conductor.

Exp. 3. I presented the *convex* side to the flame of a candle, but not so near as to touch the flame, and held it there about one minute, during which time the stone acquired a *plus* electricity on both sides, for

* I did the same thing in every experiment where there was occasion to use these balls, to be more certain of the conclusions. •
the

the balls *receded* from them, but rather with a greater force from the *convex* side, than from the *plain* side.

This appearance proves an *increase of the power in the stone*; because it continued to act for a time like other bodies electrified *plus*. And in regard to the different forces of the two sides, that will be particularly considered hereafter.

Exp. 4. After a short time, the *Tourmalin* being colder, and the remaining heat more equally diffused, it changed its last state to a *plus* and *minus* one; for the *plain* side made the balls *accede*, and the *convex* side made them *recede*.

This change seemed to arise from some alteration, on or near its surfaces, by having the heat equally diffused throughout the stone: if it was not so, I see no reason but that the *Tourmalin* should continue to be electrified *plus* on both sides, whilst any electrical signs remained.

Exp. 5. I now held the *plain* side as near the flame as I had done the *convex* one; and instead of both sides being electrified *plus*, they were electrified *minus*, for each side caused the balls to *accede*.

From this experiment, I had reason to believe that the *stone* was *emptied* (if I may so say) of its electricity; because it continued to act for a time like other bodies electrified *minus*.

Exp. 6. After the same length of time as in the fourth experiment, the *Tourmalin* being colder, it changed its state also; for the *convex* side made the balls *recede*, and the *plain* side continued to make them *accede*, as they had done before.

This

This change seemed likewise to arise from some alteration on the surfaces of the *Tourmalin*, by having the heat equally diffused throughout the stone: if it was not so, I see no reason but that the *Tourmalin* should continue to be electrified *minus* on both sides, whilst any electrical signs remained.

I was naturally led next to examine at which surface of the *Tourmalin* the fluid entered (if any did), whilst it continued heating.

Exp. 7. *Flame* being improper for my purpose, because the electric fluid is readily dissipated by its presence, I made choice of an iron rod, at the end of which was a round knob. This was heated, and afterwards brought to a certain distance from the balls, in order to see if they were moved by it: but not perceiving the least motion, I interposed the *Tourmalin*, with the *convex* side next the balls. They *acceded* a little, and when I removed the heated iron, they returned to their place again. I then brought the iron nearer to the *Tourmalin* than before: the balls in this case *moved* with vigour towards the stone, and continued in *contact* with it for a considerable time; and, after that, they *receded* from it.

On examining the balls, I found they had lost all their *plus* electricity, and were electrified *minus*. I also observed that the *stone* itself was *minus* on both sides.

I gathered from this experiment, that the *electric fluid* flowed from the balls towards the stone; because they not only lost their own *plus* electricity, but were electrified *minus*: and, as the *Tourmalin* was *minus* also on both sides, a quantity of electric fluid must have

have flowed from it towards the iron. That this was really the fact will appear presently.

Exp. 8. When I heated the *convex* side in the same manner as I did the *plain* one, the balls were not moved towards the *Tourmalin*, but from it, and continued in that state. In this experiment they were electrified *plus*, and the stone also was *plus* on both sides.

I gathered from this experiment that the *plus* electricity in the stone did not flow from the balls, because they lost none of their virtue; but it must be *from the iron itself*.

It was now necessary to examine the iron: but I found it very difficult to do this for many reasons, and such as might perhaps be thought too tedious if I mentioned them.

I therefore had recourse to another expedient, where there was a probability of meeting with better success. This was to make use of a tube of glass about two feet long, one end of which I heated red hot and tried the experiments again: observing at the same time the state of the glass after each experiment.

Exp. 9. When the *plain* side of the *Tourmalin* was exposed to the heated end of the glass, in like manner as it was to the knob of iron in the seventh experiment, I observed that about *three inches* of the heated part of the glass was *electrified minus*, and beyond that distance it was *electrified plus*, and continued so even when the glass was very near cold.

This *plus* and *minus* state in the glass, must be caused from the action of the fluid flowing from the

balls and *Tourmalin* towards the glass: because I found that a current of the electric fluid acting against the natural quantity of the fluid in the glass will produce the same effect: for having rubbed a tube of glass, I applied it to the heated end of another tube, and the appearances were exactly alike; that part which was heated being *minus*, and the part beyond it *plus*.

This seems a leading step, towards discovering the truth we are in search after: for the current of the fluid seems to be fairly traced in these circumstances, the heat having disposed the *Tourmalin* properly to let the fluid pass *from the balls, through its substance, towards the glass*.

Exp. 10. I was now eager to try what would be the event, when the heated part of the glass was next the *convex* side of the *Tourmalin*. Upon making the experiment, I found that the tube was electrified *minus above one foot in length*; without the least appearance of a *plus* electricity beyond the *minus* one, as in the last experiment. And this *minus* appearance continued also when the tube was nearly cold.

Now because the *Tourmalin* was *plus*, the balls *plus* also, and the heated glass *minus*, the electric fluid *must have flowed from the glass to produce a plus electricity in the stone and balls*.

Thus you see, Sir, I have discovered *two currents of the electric fluid* passing in contrary directions, the *one of them electrifying both sides of the Tourmalin plus, and the other both sides minus*.

MY next step was to put the *Tourmalin* into its natural state (as *Æpinus* calls it,) in order to make farther experiments.

Exp. 11.

Exp. 11. To answer this purpose best, I separated the *Tourmalin* from the wax, and placed it in boiling water for a short time, where it was surrounded on all sides with a conductor and an *equal degree of heat*: then, taking it out of the water, I laid the *convex* side (after it was dry) upon the slip of wood, supported by wax, to which the pith balls were suspended; but no appearance happened, for the balls continued at rest.

Exp. 12. But when the *stone* remained on the wood a little time longer, the balls separated to a considerable distance, sometimes near two inches; and remained so for more than one minute.

In this state they were electrified *plus*, as appeared by their acceding towards amber when it was rubbed and brought near them. From the balls being electrified *plus*, the flow of the fluid from the *plus* side of the *Tourmalin* must have caused some part of the wood itself to be *minus*, and the balls *plus*; because the same effects happened in *Exp. 9.* Besides, electrified glass opposed to the same wood produced similar appearances.

Exp. 13. If, whilst the stone continued resting upon the wood, I brought my *finger* near the *plain* side of the *Tourmalin*, the balls *receded farther* from each other; and when I repeated the approach, it every time affected the balls and made them recede a little more, unless the *Tourmalin* was become too cold; in which case they approached nearer, but still continued to be electrified *plus*.

In this experiment, the *finger* did nothing more than supply the stone with the electric fluid, more readily than the air itself would have done.

Exp. 14. Upon removing the stone *by degrees* from off the wood, the balls approached nearer and nearer: but when it was taken away entirely they receded again, and in this case were electrified *minus* instead of *plus*.

This is a farther confirmation that the fluid flowing from the *stone* electrified the wood *minus*, by forcing part of the natural quantity in the wood into the balls, and so made them *plus*; but because some part of the fluid was forced likewise out of the balls into the air whilst they continued *plus*, therefore when the *stone* was taken away, they were just as much electrified *minus*, as the force of the fluid flowing from the *stone* was able to drive out of the balls: and I have formerly shewn * that the balls will *recede* from one another in this *minus* state as they do in a *plus* one, by the crowding in of the fluid from the air &c. on all sides to restore the *æquilibrium*; but being retarded in some degree in endeavouring to enter the balls by the *medium* on their surfaces, an accumulation is formed, and consequently atmospheres similar to the *plus* ones; with this difference only, that one is tending from the body and the other to the body.

Exp. 15. After heating the stone again in boiling water, I laid the plain side upon the wood. In this case the balls continued at rest as they did in the eleventh experiment, when the *convex* side laid upon the wood.

* See exp. and obs. by Hoadly and Wilson.

Exp. 16. But, after a little time, the balls separated an inch or more, and remained so for some time. In this state they were electrified *minus*; for they *receded* from the amber.

The *Tourmalin*, in these circumstances, was supplied with the electric fluid from the wood and balls, as appears from the preceding experiments, when the heated glass was applied; so that the balls must have been in a *minus* state, and some part of the wood in a *plus* one: because of the like resistance at the surface to be overcome, where an accumulation of the fluid must have been caused before it could pass to the *Tourmalin*; as appears by Lord Charles Cavendish's experiment.

Glass electrified *minus*, and applied in the same manner, produced the like effect. As to the method of electrifying glass *minus*, it will be shewn presently.

Exp. 17. Upon bringing my *finger* near the *convex* side, the balls *receded farther from each other*, as they did in the thirteenth experiment: and, on repeating the approach, the balls *receded* a little more, unless the *stone* was become too cold.

The approach of the *finger*, therefore, conducted the fluid from the *stone*, more readily than the air surrounding it.

Exp. 18. On removing the *Tourmalin* the least from the wood; the balls approached nearer each other, and continued to do so as the *stone* was removed farther off; nevertheless they were electrified *minus*, though in a less degree.

Exp. 19. I then removed it entirely, and the balls *receded*, but to a greater distance than at any time before

before in the preceding experiments ; and, instead of being *minus*, they were now *plus* ; for the amber caused them to *accede*.

By the balls *receding* to a greater distance in this last case than in any other experiment I have yet produced, and the flow of the fluid, during the *natural state* of the stone, being from the *minus* to the *plus* side, *we have produced another proof, that the resistance is least at the minus side of the stone* : and from this cause, the tendency of the fluid from the balls towards the *stone* must be greater, than when the fluid tended from the *stone* towards the balls. But there is a resistance appertaining to the wood, that was observed before, and which must be taken into the account, though it is the same in each experiment. Nevertheless, since the tendency of the fluid is different when different sides of the *stone* are exposed, different degrees of the electric fluid will be accumulated. Hence we see the reason why, upon removing the *stone* from off the wood, as in this last experiment, *more* of the accumulated fluid must have *flowed in*, than *flowed out*, because the balls were electrified *plus*.

HAVING endeavoured to explain these *three different states* of the *Tourmalin*, caused by different applications of *heat*, I shall now offer some farther experiments in which *friction* is concerned, and compare them with other experiments of the vitreous and resinous kind, in order to observe how far they agree with one another, and whether the principles here advanced are constant and uniform.

Exp. 20. The *Tourmalin* being again fixed to the wax, I gave the *convex* side one slight rub with my
finger,

finger, and observed, that both sides were electrified *plus*.

When the *Tourmalin* was put into its *natural state*, so as to electrify *plus* and *minus*, I gave the same side another slight rub; and in this case both sides were electrified *plus*.

Upon repeating these two last experiments with the *plain* side instead of the other, the *Tourmalin* was electrified *plus* on both sides likewise; but with this difference, that now they were considerably more electrified than before.

This is a farther argument, that *the resistance is less on the plain side than on the convex side*; and that *the fluid passed through the stone*.

And because so *slight a friction* occasioned such a sensible alteration, we are sufficiently cautioned from *touching* the stone at any time, but when the experiment requires it. The same caution is to be observed with glass, amber, silk, &c.

These experiments put me upon trying the experiment with the pane of glass, mentioned in the beginning of this letter; and, upon finding that the *electric fluid* not only *passed through the glass*, but electrified it *plus*, I had a mind to try, whether I could not electrify it *minus*.

Exp. 21. For this purpose I made use of the same glass, and, when it was a little warmed, I held it within two feet of the prime conductor, which was electrified *plus*. By this method, that part of the glass, which was opposed to the conductor, became electrified *minus on both sides*; but beyond that, a considerable part all round the *minus*, was electrified *plus on both sides*. This effect is of the same kind with

that mentioned in the 9th experiment. In a few minutes, the *minus* electricity disappeared, and the *plus* continuing, diffused itself into the place of the other ; so that now the *whole* was electrified *plus*.

Exp. 22. The experiment so far succeeding, induced me to make use of a less piece of glass, that I might have the *whole electrified minus*. Upon making the experiment, it answered accordingly.

These advances led me to observe the power of electrifying this small piece of glass and the *Tourmalin* at different distances.

Exp. 23. I exposed the small piece of glass to the prime conductor, at the distance of two feet, which was the same as before in the 21st experiment, and observed a *minus electricity at both surfaces*.

Exp. 24. As I moved the glass *nearer, to a certain distance*, it was more sensibly electrified *minus* ; and after that, on moving it still nearer, the *minus* appearance was less and less sensible, till it came within the distance of about *one inch*, and then it was electrified *plus on both sides*.

These last experiments are farther confirmations of the *permeability of glass*.

Exp. 25. This *plus* electricity in the glass I found might be changed to a *minus* once again, by removing the glass, and holding it for a time at a greater distance : which is another proof of the *repulsive power of this fluid*.

Exp. 26. The *Tourmalin* afforded like appearances when in the same circumstances ; with this difference, that they were caused at *greater* distances than those of the glass ; and particularly the *plus* electricity was acquired at the distance of *one foot*, or more.

From

From this difference in the power at different distances, I inferred, that the *Tourmalin* resists the exit and entrance of the fluid considerably less than glass, or even amber; for the distances requisite to cause changes in these, were less, than the distances which caused changes in the *Tourmalin*: and since, by exposing glass, and the *Tourmalin*, to an electrified body, at considerable distances, they are rendered *minus*, and at a nearer distance *plus*; it is no inconsiderable argument that their *general laws are the same*; and that the *Tourmalin* differs in nothing from other electric bodies, but in acquiring an electricity by heat. And, in regard to this remarkable effect, the experiments I formerly made, which rendered electrics non-electric, as likewise Mr. *Delaval*'s curious experiments upon *earthy* substances, are other instances how particular bodies may be so altered, as to suffer the electric fluid to pass through them or not, according to the different degrees of *heat* employed in the experiment.

But to proceed with our observations on rubbed glass.

Exp. 27. Having by me a pane of glass, one side of which was rough, and the other smooth, I rubbed it slightly on the rough side; upon doing which, *both sides were electrified minus*.

Exp. 28. I treated the other side in the same manner; after which, the *minus* electricity was changed to a *plus* one, on both sides.

Now, because the same glass afforded different appearances when different sides thereof were rubbed, and there being no other difference in the circumstances of the two experiments, except that in the surfaces themselves, the one being rough, and the

other smooth; it follows, that the power of electrifying *plus* or *minus* arises from one and the same fluid.

Exp. 29. I then had a curiosity to try, whether I could not, by rubbing, make one side of this glass *plus*, and the other *minus*, at the same time. This I effected, after both sides were made *plus*; for, by rubbing the rough side of the glass less, than I rubbed the smooth side, that became *minus*, and the smooth side continued *plus*. I rubbed the rough side less, because I found from experience, that rough glass required a less power to electrify it *minus*, than smooth glass did to electrify it *plus*; and therefore I concluded, that the medium on the different surfaces has different powers, the greatest belonging to the smooth, and the least to the rough surfaces; as Sir Isaac Newton has shewn concerning light falling upon polished and rough glass.

I remember an observation of the like kind, which Mr. Short made, upon having occasion to heat one of his *metal speculums*, behind which was fastened a wooden handle. This *speculum* he placed near a strong fire, with the polished surface towards the same, where it continued above an hour, without receiving the least degree of warmth. That power therefore, which reflected the heat, must certainly be of the same nature with that, which occasioned the knobs of light in *vacuo*, mentioned in the first experiment. I have frequently endeavoured to cause alterations in that power, with a view to be better acquainted with its laws; and, amongst other attempts, I rubbed electrics against electrics.

Exp. 30. The first trial was with the *Tourmalin* and amber, which produced a *plus* electricity on both

sides of the *stone*, and a *minus* one in the amber. Afterwards I electrified the amber, and held it near the *Tourmalin*; still both sides were *plus*: and if I rubbed the *Tourmalin* whilst the amber was electrified, it continued *plus*. Then I rubbed the *stone* with glass; notwithstanding which, both sides of the *Tourmalin* were *plus*, and the glass *minus*.

Exp. 31. But when the glass was electrified *plus*, and held near the *Tourmalin*, as I had done before with the amber, in this case, both sides were electrified *minus*.

These experiments seem to shew, that *where electric appearances are produced, by the rubbing of any two polished bodies together*, that body, whose substance is *hardest*, and *electric power strongest*, will be *always plus*, and the *softest* and *weakest*, *always minus*. It was from this theory, with which I had the pleasure to acquaint you, that I was desirous of trying to electrify the *Tourmalin minus* by *rubbing*, not having at that time been able to do it. I fixed upon a *brilliant diamond* for this purpose, as being the *hardest* body, and *strongest electric*, that I was acquainted with; and, upon rubbing the *Tourmalin* with it, my expectations were answered; for both sides of the *Tourmalin* were electrified *minus*, and the diamond *plus*.

Exp. 32. These experiments succeeding, I rubbed glass against glass, and found, that *they electrified each other*; but one of them was *plus*, and the other *minus*.

Exp. 33. Two pieces of amber, treated in the same manner, were also electrified *plus* and *minus*.

Exp. 34. When I rubbed glass against amber, the former was *plus*, and the latter *minus*.

Exp. 35. Two *Tourmalins* being rubbed against each other, one became *plus*, and the other *minus*.

From all which it appears, that some alteration was made in the *medium* on their surfaces; otherwise these opposite effects could not have been produced: and in regard to the same bodies producing different effects, it is not improbable but they may differ in degrees of hardness, polish, or of their electrifying power.

Now, as *electrics*, rubbed against *electrics*, occasioned *electrical appearances*, I was encouraged to try what would be the effect of *air*, if I *rubbed*, or rather *forced*, it against *electrics*; for I supposed the particles of air to be surrounded with a *medium* of the same kind as grosser bodies, which is the cause of their being so elastic.

Exp. 36. To do this, I only made use of a common pair of *bellows*, and having brought the *Tourmalin* near to the end of the pipe, I found, after it had received about *twenty blasts*, it was *electrified plus* on both sides.

Air therefore seems to be *less electric* than the *Tourmalin*.

Exp. 37. Into the place of the *Tourmalin* I brought a pane of glass, and blew against it the same number of times as in the former experiment. When I examined both sides, they were electrified *plus* also, but less than the *Tourmalin*.

Exp. 38. Amber, treated in the same manner, was electrified less than the glass.

Exp. 39. I had recourse next to a smith's *bellows*. The difference these occasioned was only a much stronger

stronger electricity in the *Tourmalin*. Amber was still weaker than the glass, and the glass weaker than the *Tourmalin*.

Still having in view the *medium* on the surfaces of the particles of air, I considered, that *heat* would rarify it; by which means, air, having its resistance lessened, would more readily part with the electric fluid, and of consequence electrify more powerfully.

Exp. 40. The *pipe* of the *bellows* being made red-hot, I blew against the *Tourmalin* twelve times only, which was eight blasts less than in the former experiments with cold air. In this experiment likewise the *Tourmalin* was electrified *plus* on both sides, but to a considerable degree more, than was done in the 36th and 39th experiments. The hot air had the same effect upon glass; but electrified it less than the *Tourmalin*: and amber, though, like the other bodies, it suffered an increase of power by the same treatment, was electrified the least of all.

From the air electrifying more powerfully when it is hot, than when cold; and the *Tourmalin* being electrified more than glass, and glass more than amber, as appears by these last experiments, we seem to have obtained a proof, that the whole atmosphere is constantly promoting a flow of the electric fluid, by the alternate changes of heat and cold: and further, that air is not only less electric than the *Tourmalin*, but less than glass, and even amber.

Exp. 41. When the *Tourmalin* had received the same number of blasts against the *plain* side, whilst my *finger* touched the *convex* side, it afforded different appearances; for the plain side was electrified *plus*, and the other *minus*. After a short time, *both sides*

sides were plus; and some time after this the *stone* recovered its *natural state*, the *plain side* being *minus*, and the *convex plus*.

What appeared singular in this experiment, was, that the *middle state of the stone* should be *plus on both sides*. But we no more wonder at this, when we consider, that there were two causes to produce these effects: the first was *heat*, which put the *stone* into an *unnatural state* (as *Æpinus* had observed before); for, upon cooling, it would recover its *natural state*, and consequently afford different appearances.

But the second cause, which electrified both *sides plus* when the *stone* was in an intermediate (or, as *Æpinus* calls it, *neutral*) state, between the two extremes, was, that then the effects of the air itself took place, and electrified *both sides plus*, as it had done before in the 35th experiment.

Exp. 42. The convex side was now presented to the *bellows* in the same manner, and received an equal number of blasts. In this experiment *both sides were plus*, but weaker than in the last experiment; and, after a time, the *stone* returned to its *natural state*, affording a *plus* and *minus* appearance.

This property in the air, of electrifying glass, amber, &c. will, in all probability, account for an experiment I met with in *France*, made by *M. Le Monnier**, who shewed me, at *St. Germain en Lays*, two or three long wires, which he had suspended horizontally from the palace to his apartment, above

* See the *Memoirs of the Academy of Sciences*.

thirty feet from the ground (the same experiment that was made by my friend the *Abbé Mazzeas*, and communicated, with several others, to the Royal Society *), in order to observe the *electric effects during thunder storms and cloudy weather*. These wires he frequently found *electrified in a small degree, when the day was clear, and without the least appearance of a cloud*. Might not this effect therefore arise from the *friction of the air against the wires*?

From considering all these things, and what these last experiments have taught us concerning the different effects produced by *hot and cold air*, it seems probable, that many of the curious operations in nature arise from a *constant flux and reflux of the electric fluid*. And if the observation be true, that air, free from moisture, during tempests and hurricanes in the night, frequently affords a faint kind of light, resembling what is seen in an exhausted receiver, through which the electric fluid is caused to flow, as also it occasions a more general clearness in the heavens, than would appear were there no such violent agitations; it is reasonable to imagine, that a *flux of the electric fluid in the air*, is the cause of such appearances; for the same reason that *our artificial blasts* produced the electrical effects I have just now mentioned.

But it remains to be inquired into, whether the *Tourmalin* is so disposed by nature, as to suffer the electric fluid to pass through it only in one direction, like *magnetism* through the *loadstone*, or is indifferent which way it flows.

* See the Transactions of the Royal Society, Vol. XLVIII.

Exp. 43. Upon examining a *Tourmalin* which was flat on both sides, and polished, except upon its edges, part of the edge appeared *plus*, and another part opposite to it *minus*; so that a line, drawn from the *plus* part through the centre of the stone to the other side, would pass through the *minus* part.

Exp. 44. Two smaller *Tourmalins*, that were flat also, and polished, like the other, exhibited the same appearances.

Exp. 45. Another *Tourmalin*, which was also flat, but unpolished, afforded a fourth instance of this kind.

Exp. 46. The first of these *Tourmalins* was afterwards polished, as well at the edges as the surface; in order to see, whether that would make any alteration; but I found it still retained its former electrical state.

Exp. 47. I experienced the same with another *Tourmalin*, which had been rough likewise.

Exp. 48. I then, with a little emery, made that edge, which was *plus*, rough again, preserving all the rest smooth; but I could not perceive, that any alteration was made by it. I did the like with the edge of the other polished *Tourmalin*, without being able to observe any difference.

Exp. 49. As to the small *Tourmalin*, that is plain on one side, and a little convex on the other, which you observed to be a very good one, it is *plus* on the plain side, and *minus* on the convex; which is contrary to the large *Tourmalin* described in the beginning of this letter.

Exp. 50. I had an opportunity of trying another *Tourmalin*, now in the *British Museum*, which afforded another instance of the singular disposition of this

this *stone*. The *Tourmalin* I now speak of, is *plain* on one side, and *concave* on the other; with a small flat border round it. It was square, but one of the corners had been broken off. When I examined it, the broken part was *plus*, and the corner opposite to the broken part *minus*: so that here also the electric current ran through the stone in a diagonal line.

Exp. 51. Every one of these *Tourmalins*, except that in the *British Museum*, I greased all over, and, whilst they were warm enough to preserve the grease liquid, I tried each *Tourmalin* separately, but found, no alteration in the virtue of the stone, except weakening it a little; though it is well known, that moisture, of any sort, readily conducts the electric fluid; and therefore, if the *Tourmalin* had not a fixed kind of electricity, the *plus* and *minus* observeable on the two sides of the stone, must, by this treatment, have united, and destroyed each other: the *plus* side parting with as much of the fluid, as the *minus*, on the other side, wanted, to restore the *equilibrium*.

Upon the whole, all these experiments do most clearly prove, that the *Tourmalin* suffers the electrical fluid to pass through it only in one direction, and so far it bears some analogy to the loadstone. And as the loadstone loses its virtue by being made red-hot, I was desirous to see what would be the event in the *Tourmalin* under the same treatment.

Exp. 52. I therefore put one of the flat *Tourmalins* into a strong fire, for half an hour; but could not, afterwards, perceive the least alteration. I made the same experiment upon another *Tourmalin*, with the same success.

Exp. 53. Lastly, I heated the stone again, and, whilst it was red-hot, I threw it into water; by which treatment the *virtue of the Tourmalin was intirely destroyed*, and it had the appearance of being shivered in many parts, without breaking.

IN regard to the internal frame of the *Tourmalin*, we can say nothing; yet so much I have learned, by these experiments, that there are *three different methods of heating the Tourmalin*, which produce different electric appearances; that *different degrees of heat*, afford different appearances; that *friction* has the same effect upon it, as upon glass; and that the *Tourmalin*, when it is heated properly, suffers a current of the electric fluid to pass through it *in one direction only*: so that the *Tourmalin* hath, as it were, *two electrical poles*, which are not easy to be destroyed or altered; and farther, *that there is not any substance in nature, which we are acquainted with, that the electric fluid does not readily pass through*; that there seems to be *a constant flux and reflux of it in all bodies, as well in the air as in vacuo*, occasioned by the alternate changes of *heat and cold* in every part of this globe. All which things do very much confirm the opinion I formerly entertained, and attempted to prove by experiment, that the *electric fluid is diffused throughout the whole earth*, as well as in the *air* surrounding it*.

How far this flowing of the electric fluid may be concerned in the ordinary operations of *nature*, by keeping up that motion, which seems so necessary in the several parts of the *grand machine*, I leave to others, who may be more successful in their researches.

* See a Treatise on Electricity, the 2d Edit. by B. Wilson.

By these advances, we have likewise attained to a more certain knowlege of that *medium*, which seems to *surround the surfaces of all bodies*, and which, being of a greater or less density, or exercising (if I may so say) a greater or less resistance, produces different effects, as the *electric fluid* (or *æther*, if you please) *passes in and out of bodies*, whenever they are disturbed by any external violence; and, were it not for this *medium surrounding all bodies*, the electric fluid, I apprehend, could *neither be accumulated, nor detained, in any body* whatsoever.

This principle is very simple, and seems to be of a very general nature. Our great *philosopher* adopted it from many experiments: he supposed, that the wonderful phænomena of *nature*, particularly those of *light*, were not to be explained without it; and therefore did not scruple to propose it, as a principle, to be farther inquired into.

I shall think my time well employed, if, by these inquiries, I have at all contributed to shew, how much we are indebted to this happy *Interpreter of Nature*, and have afforded fresh occasion, by the light of his labours, to admire, and adore, the *first cause of all things*. But whatever may be the success of my endeavours, they have at least been attended with a satisfaction, which not a little increases that desire I have ever had for pursuits of this kind. I am,

S I R,

Your most obliged humble servant,

Great Queen-street, London,

Benja. Wilson.

Nov. 9th, 1759.

XXXVI. *New Experiments and Observations concerning Electricity*; by Robert Symmer, Esq; F. R. S.

P A P E R I.

Of the Electricity of the human Body, and the Animal Substances, Silk and Wool.

Read Feb. 1. 1759. **I** had for some time observed, that upon pulling off my stockings in an evening they frequently made a crackling or snapping noise; and in the dark I could perceive them to emit sparks of fire. I made no doubt but that this proceeded from a principle of electricity; and I was confirmed in this opinion, by observing that, in weather favourable for electrical experiments, those appearances were more remarkable than at other times. I mentioned this observation to several of my friends, and some of them told me, they likewise had often perceived the snapping, and the emission of fire from their stockings upon pulling them off, especially in the winter evenings: but I could not hear of any body that had taken this phenomenon into consideration in a philosophical way. For my own part, I could not but think that so striking an appearance, one that seems to have an immediate connexion with the human body, or is at least as much about us as the cloaths we wear, and is obvious to the perception of our senses, merited not only a philosophical attention, but the strictest inquiry possible. I was the more induced

duced to such an inquiry, as it seemed to me to open a new path for proceeding in electrical researches, and might perhaps throw new light on the great and surprising discoveries already made in that branch of natural philosophy. The simplicity of the apparatus, and the great facility in making the proper experiments, putting it in my power to begin and carry on my enquiry at pleasure, I entered upon somewhat of a regular course of observations about the beginning of November last; and since that time have pursued it as closely as my leisure, and the weather, which has been far from favourable for electrical experiments, would permit. It is the purpose of this, and of those papers that may follow, to lay before the Royal Society an account of the experiments and observations I have already made, or may hereafter be able to make, in the progress of this inquiry.

My first endeavour was to discover what sort of stockings was most proper to produce electricity. In order to determine this, I tried single stockings of different kinds, *viz.* thread, cotton, silk, and worsted, putting them on, and wearing them some time. On pulling them off, I could perceive nothing of electricity in the thread or cotton, and no remarkable degree of it in the silk and worsted.

When I say *no remarkable degree of it*, it may be proper to be a little more explicit. I must therefore observe, that silk and worsted, being in themselves electric, are both of them, especially silk, extremely susceptible of electricity. I have sometimes observed, particularly when the weather was favourable, that ~~silk~~, when but barely handled, nay, when but simply touched, has become electrical where it had been handled.

handled or touched, as appeared by its attracting little light balls suspended by threads. It cannot therefore be supposed that silk or worsted stockings can be put upon the leg, without being excited to a sensible degree of electricity. And thence it is, that when taken off they are sometimes perceived to snap, though worn single. But whatever electricity the single stocking acquires by friction or otherwise, it immediately loses upon being separated from the leg: if any electric virtue remains, it is no more than what belongs to it as an electric substance ceasing to be excited; and it is in so small a degree, as in the present case not to merit attention. In general, when I speak of the electricity in question, I mean such a power of electricity as is obvious, and perceptible to the senses; so that the stocking, after being taken off, should appear more or less inflated; throw out an electrical wind to be felt by the bare leg; attract or repel another stocking visibly; and, upon the touch, snap, and emit, or receive electrical fire.

After making the experiment above-mentioned with single stockings, I proceeded to try the effect of two stockings upon one leg. This I did with two of thread, cotton, worsted, and silk successively; but this produced no electrical appearance more than before. I then combined them one with another, and, running through all the different changes, I found that none of those I then made use of exhibited visible proofs of electricity, but the silk and worsted together; and there, indeed, the electric power appeared remarkably strong. It seemed to be a matter of indifference whether the silk or the worsted was uppermost, the combination of the two was what I judged

judged to be essential; and happening at that time to wear silk stockings with thin worsted under them, I kept to the use of these, and found it a convenient circumstance in the course of my experiments.

As some may have the curiosity to examine the observations I present them with, by experiments of their own making; before I proceed further, I shall furnish them with a few remarks, which may enable them to make their experiments with more ease and certainty, than if they set out unacquainted with some circumstances, which I have learnt from experience.

One of the first things to be attended to, is the weather, which has an influence on all electrical experiments, but upon none more than those which relate to this branch of electricity. The most favourable weather is that which is dry and clear, and, if a little frosty, so much the better. In general, when the quicksilver in the barometer rises, and the fire burns remarkably brisk and clear, we may expect a considerable appearance of electricity: at other times, it is better not to attempt the experiment.

When the weather is favourable, it is not necessary to wear the stockings all day: That, indeed, raises the electrical power to the highest degree; yet, provided they be dry, and made warm by the fire, before they are put upon the leg, their continuance upon it for a very short time, prepares them sufficiently for exhibiting visible proofs of electricity. If this should not answer upon the first trial, the operation may be repeated two or three times, to bring them into a proper disposition to receive the electrical virtue; and this is no more than what is found to be sometimes necessary

necessary with respect to glass globes and tubes, especially when new. If the stockings have once acquired this disposition, they retain it for the day, or until a change of weather, and the experiment may be repeated with success as often as one pleases; for no sooner are they discharged of their electricity, than they are ready to receive it anew; nay, if speedily returned upon the leg, they catch it instantaneously, and may immediately be pulled off to exhibit a new explosion: what is still more surprising, they seem to gather force by the frequency of the repetition, and, to some certain degree, increase in electrical power, provided they be all along kept warm and dry, and that the leg continue warm.

Another circumstance to be carefully attended to, is the manner in which they are to be taken off; for as to the putting of them on, it is a matter of indifference how that is performed. In taking them off, care must be had not to separate them; for if that happens in pulling them off, all the electricity escapes. The best way, is to put the hand between the leg and the stockings, and push them off together. Nothing more remains to be done, than to pull them asunder; for upon that, they both of them exhibit a degree of electricity, which, when at the highest, is really surprising.

Before I conclude these occasional remarks, it may not be improper to observe, that it is not absolutely necessary that the stockings be applied to the bare leg; if a fine thread stocking separate them from the leg, though left upon it when they are taken off, it does not much impair their electricity. But it is more convenient to use the hand instead of the leg. The
 insertion

insertion of the hand into the stockings, is alone sufficient, especially in favourable weather, to communicate such an electric power, as renders them capable of answering expectation, in such experiments as I have had occasion to make*.—I now return to my subject.

The opinion I had conceived, that the combination of silk and worsted was necessary for the production of electricity, seemed to meet with confirmation from all the experiments I made with the stockings I then wore. I was at that time in mourning, so that my silk stockings were black, and under them I constantly wore thin white worsted. About the latter end of November I went out of mourning, and of course changed the colour of my stockings. On the second of December, having put on a pair of white silk above the worsted, after I had wore them some hours, I resolved to amuse myself with a few experiments. The weather was remarkably favourable, and I had reason to expect a fine appearance of electricity: but upon taking off my stockings, and pulling them asunder, to my great astonishment, I found they discovered no signs of electrical power; as I

* An improvement may perhaps be made upon this, by getting a piece of silk or worsted knit, or wove in the stocking loom, so that without being cut, it may be formed into the shape of long mittens or sleeves, or rather into what we might call a silk or worsted tube. The reason why it ought to be knit, or wove in the stocking loom, is, that it may better embrace the hand or arm; and likewise, that it may be more retentive of electric virtue, which would escape more easily from a piece of silk or worsted wove in the common loom, and cut with a number of ends of threads exposed, than from any thing formed in the fashion of chain work, and consisting of one single thread of silk or worsted.

held them in my hands they hung down collapsed, and did not more attract one another, than before they were put upon my leg. I repeated the experiment two or three times, but with no better success. An event I so little expected, disconcerted me much. I saw I was no longer to ascribe electricity to the combination of silk and worsted; but I remained at a loss to know to what I *should* ascribe it. At last, upon considering the circumstances of this and other experiments, a conjecture occurred, that the electricity in question might depend upon the nature of different colours. In order to determine this, I thought it fairest to make the trial in the same substances. Accordingly I had recourse to the following experiment.

I took a pair of white silk stockings, and having warmed them at the fire put them both upon the same leg. After I had worn them about ten minutes, I took them off, and pulled them asunder, but discovered no signs of electricity in either. I did the same with a pair of black silk, but to no other effect. I then proceeded to the decisive trial. I put a black and a white stocking upon my leg, and wore them likewise ten minutes. I waited with some impatience to see the success of my experiment, and in return had the satisfaction of observing, upon their being pulled asunder, that each of them had acquired a stronger degree of electricity than I had before seen: they were inflated so much, that each of them shewed the entire shape of the leg, and at the distance of a foot and a half they rushed to meet each other. I went through the same experiment with worsted stockings, and found that, as in silk, nothing but the combination of black and white produced electricity.

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As I had often experienced the power of electricity in the combination of black silk with white worsted stockings, there remained to try but that of white silk with black worsted, which answered as I expected, and seemed to complete the demonstration.

A phenomenon so new, and of so curious a nature as this experiment presented, could not but for some time engage my attention. I saw, that if this appearance did not arise from some accidental or collateral cause, correspondent effects would follow, upon the combinations of the intermediate degrees of light and shade, between the extremes of white and black. I have had a particular attention to this in the experiments I have since had occasion to make, and so far as I have yet been able to go, it appears to answer my expectation. Nevertheless, as this is a matter that merits a more minute examination, we may hereafter take it up as an immediate object of inquiry; with a view to determine, if light and colours have, of their own nature, a relation with electricity, and in what that relation consists.

In the mean while I shall pursue my principal design, which is to investigate from experiments the nature of that electric virtue, with which the animal substances under consideration appear to be endued. I have already made some progress, farther than what is contained in this paper; and I shall continue to follow the views that seem naturally to open to my inquiry. If I make any discoveries, or come at any conclusions that may merit the attention of the curious, they shall, together with the experiments on which they depend, be submitted to the judgment of this learned Society.

New Experiments and Observations concerning Electricity.

P A P E R II.

Of the Electricity of black and white Silk.

Read May 17, 1759. **I**N my former paper, I gave the Society an account of some experiments, made with silk and worsted stockings; by which it appeared, that the remarkable degree of electricity they had acquired, by being put upon the leg, depended on their being of different colours, namely, black and white. I did not then, nor do I yet, take upon me to determine the cause of this phenomenon. Whether it be owing to light, which is the origin of colours; or only to the ingredients, which enter into the composition of the several dyes; or to those conjunctly with the colours they produce: in any of these views the matter is curious, and equally merits a careful inquiry.—But I fear the solution of this, and of many other difficult questions concerning electricity, will depend upon the establishment of a more perfect theory than we have yet attained to. Till we shall be so fortunate as to discover the nature and properties of the powers employed in these operations, we must be satisfied to pursue the path of experiment and observation, in quest of those, as first principles. This is the method I shall continue to follow; and having already shewn the manner of electrifying the black and the white stocking, it is now my purpose to give an account of the appearances and powers of that electricity so excited.

Having made a great number of experiments since I had the honour of presenting my former paper to the Society, I have had an opportunity of observing, that the electricity produced between black and white silk, is stronger than that between silk and worsted of those different colours, and a great deal stronger than that between worsted and worsted: the last is so weak, except in time of frost, or when a sharp north-east wind blows, that tho' the effects are always of the same nature, yet they are sometimes so languid as to be scarce perceptible. I shall therefore, and for the sake of brevity, confine myself, in this paper, to what is observable with regard to the electricity between black and white silk.

It is proper to mention another circumstance: having found it troublesome to electrify the stockings, by putting them as often on my leg as was requisite in making experiments, I have quitted that method intirely; and satisfy myself with the degree of electricity which is excited in the stockings by drawing them upon the hand: and this is to be understood with regard to all the experiments and observations I may have occasion to mention, unless when otherwise expressed. The electricity thus produced is not equally powerful with that which is excited by means of the leg; but it is nevertheless sufficiently strong to answer all the purposes in view; and it is attended with this advantage, that the stockings continue longer fit for these experiments: for, like other electrical apparatus's, they must be kept clean, and free from all extraneous matter; and are therefore most to be depended upon when new, or when newly washed.

To give a distinct account of the electricity of black and white silk, I shall trace it through its whole process, beginning before the stockings of the different colours are put together.

After being a little air'd at the fire, when the black silk is drawn single upon the hand, a crackling noise is heard; and in the dark, sparks of fire may be perceived as passing between the hand and the stocking: while it is drawn backwards and forwards the crackling continues, and is most considerable upon the separation of the stocking from the hand. Thus it appears, that black silk is highly susceptible of electricity; that it is produced almost instantaneously, or at least with very little friction; that most of it escapes, while the stocking is yet upon the hand; and that, upon the total separation, very little remains. This is similar to what happens with the glass tube, when the hand, after passing along it in one direction, repasses it in the other. But still the electricity that the stocking retains, after it is separated from the hand, is considerable enough to attract or repel little light bodies at the distance of one or two feet: some degree of inflation in the stocking is likewise perceivable; and when a non-electric is brought near it, a crackling is heard, and in the dark sparks may be seen. If two black stockings be drawn upon the hand at a time, the appearances are much the same as before; only that the stockings, when taken off and separated, give smaller proofs of electricity, than if each of them had been single upon the hand.

Having found it necessary, as I proceeded in my course of experiments, to fix upon some method of ascertaining the principal appearances of electricity;
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and marking the degree of it, I had recourse to the little pocket electrometer of Mr. Canton's contrivance, which is described in Vol. XLVIII. N^o. 93. of the Transactions of this Society. When this instrument is supported by glass, it not only shews the attraction and repulsion, in general, of electrified bodies, which is one of the most essential properties of electricity, but it distinguishes between the positive and negative state of electricity, according to the reciprocal attraction or repulsion of the little balls. By the terms *positive* and *negative*, I mean only to denote the opposition of the two different states. The particular allotment of the one or the other term appears to me to be arbitrary; but that I may not differ unnecessarily from others, I shall apply the word *positive* to that state, in which a body is found to be, when electrified by the clear glass tube, rubbed by the hand; and the word *negative*, when electrified by the rough or opaque glass tube, of Mr. Canton's invention (described in the Transaction mentioned above), when rubbed in the same manner, or by sulphur or wax excited. In other words, when the body is in a state of repulsion with the former of those tubes, we say it is *positively* electrified, and *negatively* when in a state of repulsion with the latter, or with sulphur or wax.

Nothing appears to be more wonderful than this double state: here electricity seems to counteract itself, the electrified body attracting in the one, what it would repel in the other case, and *vice versa*. As this remarkable property may be traced, in its consequences, through almost all electrical appearances, I cannot but think it merits great attention, and, when
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it comes to be better understood, may throw much light upon the system of electricity. However that may be, it was impossible for me not to observe, that it runs through the whole of that branch, which I have at present under examination, and to which I return.

White silk differs much in electricity from black silk. When the white stocking is drawn separately upon the hand, no crackling is heard, nor sparks of fire seen in the dark, let it be pulled backward and forward ever so often: when another white stocking is drawn on above it, nothing more appears: and, when separated from the hand, neither of them discovers any signs of electricity, excepting that, when brought within a few inches of the electrometer, they attract and repel the balls a little.

If instead of two white or two black stockings, one white, and over that, a black stocking be drawn upon the hand, they discover not the least signs of electricity while they continue upon the hand, even tho' they should be drawn backwards and forwards upon it several times; nor, when taken together from the hand, and presented to the electrometer, do they appear to have acquired any more than a very small degree of electricity. They must be brought within the distance of a foot, nay, sometimes of a few inches, before they have any effect upon the balls: but the moment they are separated, they are found to be both of them highly electrified, the white *positively*, and the black *negatively*. The circumstances, that appear the most to merit observation, are as follow:

1°.—When the electrometer is placed on a non-electric, and the black stocking is presented to it at the distance of 3, 4, or 5 feet, according as it happens to be more or less powerfully electrified, the balls begin to be visibly attracted, and when it is brought nearer, they are seen to be agitated in a violent manner. If, instead of the black, the white stocking be presented at the same distances, it is found to have precisely the same effects, attracting and agitating the balls in the very same manner: From whence it appears, that whatever difference there was between the electricity of the black and the white, under other circumstances, they each of them acquire an equal degree of electricity, by being electrified together.

2°.—When the electrometer is supported by glass, and the white stocking is presented to it, it first attracts the balls, and afterwards repels them; when taken away, it leaves them in a repulsive state with regard to each other; when brought back, it repels them as before. If, in place of the white, the black be now presented, the balls are immediately attracted, soon after again repelled, and left once more in a repulsive state with regard to each other. If the white be again presented, the same train of effects takes place as before; and so on, alternately, as in the case of the clear and opaque glass tubes, when excited; the white stocking answering precisely to the clear, and the black to the opaque tube, and acting the one *positively*, the other *negatively*, at full as great a distance, and as forcibly, as the tubes.

3°.—Both the stockings, when held at a distance from one another, appear inflated to such a degree,

that, when highly electrified, they give the intire shape of the leg; and when brought near the face, or any naked part of the body, there is a sensation felt, as if a cool wind was blowing upon that part. When the two white, or the two black, are held together by the extremities, they repel one another, and form an angle, seemingly of 30 or 35 degrees.

4^o.—When a white and a black stocking are presented to each other, they mutually attract, with a force answerable to the degree of electricity they have acquired: when brought within the distance of three feet, they usually incline towards one another: within two and a half, or two feet, they catch hold of each other; and when brought nearer, they rush together with surprising violence. As they approach, their inflation gradually subsides; and their attraction of foreign objects diminishes: when they meet, they flatten, and join as close together, as if they were so many folds of silk; and then the balls of the electrometer are not affected at the distance of a foot, nor even of a few inches at certain times. But what appears most extraordinary, is, that when they are separated, and removed at a sufficient distance from each other, their electricity does not appear to have been in the least impaired by the shock they had in meeting. They are again inflated, again attract, and repel, and are as ready to rush together as before*.

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* The phenomena, here remarked, of the black stocking and the white when electrified; namely, that, as they approach one another, their attractive and repulsive force decreases, with regard to foreign objects, but increases surprisingly, with regard to each other; and that their electricity suffers no diminution from the shock

When this experiment is performed with two black stockings in one hand, and two white in the other, it exhibits a very curious spectacle: The repulsion of those of the same colour, and the attraction of those of different colours, throws them into an agitation that is not unentertaining, and makes them catch each at that of its opposite colour, at a greater distance than one would expect. When allowed to come together, they all unite in one mass; when separated, they resume their former appearance, and admit of the repetition of the experiment as often as you please; till their electricity, gradually wasting, stands in need of being recruited.

5^o.—When they are separated from one another, they lose their power very soon, much as the excited tube does; but when they are together, they will retain it for an hour or two, and longer, when the air is in a state favourable for electricity. While they are asunder, and any non-electric is brought near them; if that non-electric is of a broad surface, it is with difficulty they are discharged of their electricity; but if the point of any, especially of a metallic, body, be presented, they are instantaneously deprived of their electrical virtue: but if they be in conjunction together, they retain their electricity with so much obstinacy, that even the sharpest point of metal cannot deprive them of it. In this, and in some

shock of their congress, appear to me to be observations new in electricity, and to merit attention. They seem to point out a retentive power of electricity, which takes place between electrics and electrics only; and which, I apprehend, may be found to be the cause of many curious and singular phenomena.

other respects, there appears to be such a resemblance between the Leyden phial, or the electrical pane of glass, and the black stocking in conjunction with the white, especially when the one is within the other, that I have been induced to consider them both in the same light. In both cases, the *positive* electricity is on the one side, and the *negative* on the other; and the stockings, as well as the phial, and the pane of glass, are at once electrified *positively* and *negatively*. In both cases there is an accumulation of electricity, and a retention of it, far beyond what is to be met with in a simple body, electric or non-electric. There is, however, a very remarkable difference between them in two respects. In the phial, and in the pane of glass, an explosion is always obtained by carrying on a communication between the two sides by the interposition of a non-electric; but, in the case of the black stocking and the white, I never yet have been able to procure an explosion, nor so much as a speedy discharge, by any means I could think of, while the one was within the other. I have put one hand within the innermost, and with my other have clasped the outward stocking; nay, I have thrust in my hand, and turned the stockings inside out, and, in that condition, have dashed them against the floor; and all this without procuring the least perceptible discharge. On the other hand, the phial and the pane of glass afford no opportunity of separating the *positive* from the *negative* electricity, so as to shew them intire and distinct from each other; whereas we need only pull the stockings asunder, and then in the white we find the *positive*, and in the black the *negative* electricity.

6°.—When the stockings are separated, and in the dark, upon presenting to them the point of one's finger, or any small metallic body, rounded at the end, they exhibit the appearance of electrical fire or light, according to the *negative* or *positive* state of the stocking the object is presented to. With the black, at the distance of two or three inches, there appears to dart from the end of one's finger a sprig or pencil, as it were, of fiery sparks, which dilates in its progress, and strikes against the surface of the stocking: at the same time a crackling, or snapping noise, is heard. When the first discharge is made, upon presenting the finger to a fresh part of the stocking, the same phenomenon is repeated, till you have traversed the whole length of the stocking, which, when the finger moves slowly, usually yields eight or ten distinct discharges, before it is divested of its electricity. With regard to the white stocking, the same appearances hold; but with this difference, that, instead of sparks of fire issuing from the finger, a little globule of white or blueish light is seen at the point of it; and, when the electricity is strong, that little body of light seems to break in an explosion between the stocking and the finger; and rather a hissing than a crackling noise is heard.

7°.—The electrical phial may be charged by the stockings, either *positively* or *negatively*, according as the wire from the neck of the phial is presented to the white or the black; and in the one, or the other case, the hissing, or the crackling noise, is louder than when any common wire, or non-electric body, is presented: but if the electricity of the white stocking be thrown into the phial, and upon that the elec-

electricity of the black, or *vice versa*; in that case, the phial will not be electrified at all.

The charging of the phial was among the first of my experiments with electrified stockings. By some trials I made in the month of December last, I found it would succeed. One frosty evening in that month, having thrown into a small phial, filled with quicksilver, the electricity of one black stocking, I received from the explosion a smart blow upon my finger. With the electricity of two stockings, the blow reached both my elbows; and, by the means of four, I kindled spirits of wine in a tea-spoon, which I held in my hand, and, at the same time, I felt the blow from my elbows to my breast. It may not, however, be improper to observe, that the electricity, in this case, was excited by means of the leg.

From what hath been said in the preceding pages, it is evident, that all the remarkable appearances of electricity, hitherto discovered, may be exhibited by a simple apparatus of black and white silk. But this is not all: in the course of experiments above-mentioned, something curious has occurred to my observation, of which I do not find that any notice has been taken by others.—What I mean is, a strong cohesion produced by electricity. But as this paper is already extended to a great length, I shall reserve the account of electrical cohesion for another, which I shall soon have the honour of presenting to the Society.

New Experiments and Observations concerning Electricity.

P A P E R III.

Of Electrical Cohesion.

Read June 21,
1759.

ACCORDING to what I promised in the conclusion of my last paper, I come in this to give the Society an account of cohesion produced by electricity. I had not made any great progress, in the series of experiments and observations I have already had the honour to communicate, before I perceived that the black and white stocking, when electrified, and allowed to come together, not only joined extremely close, but actually stuck to each other; insomuch that unless when weak in electricity, or improperly applied, I could make the white bear the weight of the black, or the black that of the white, and that for a considerable time. But as the weather, for some months in winter, was so seldom favourable for electrical experiments, that I had scarcely opportunities sufficient to satisfy myself with regard to other points, I did not enter upon an examination of this phenomenon experimentally, till about the latter end of March. By that time, I had got ready the scale of a balance properly fitted with a hook to catch hold of the stocking, a set of Troy-weights I could depend upon, and an exact pair of scales, to take the precise weight of the stockings as occasion should require.

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By experience I found that the same pair of stockings did not always do equally well, even independently of the weather; and that, by being too frequently electrified at a time, their virtue appeared to diminish. I therefore judged it proper to be provided with changes of pairs; and that there might be the greater conformity between the experiments, I chose them as exactly as possible of the same size and substance. The sort I fixed upon, is what is called half gauze; the weight of the white stocking, at an average, 18 *dwt.* and 10 *gr.* but when died black, 1 *oz.* and 1 *dwt.* the weight being increased, by the dying of that colour, above 5 *dwt.* in the pair. When the white and the black stocking were warmed at the fire, so as to be prepared for electricity, they usually lost about a twentieth part of their weight; so that in the course of my experiments I rate the white at 17 *dwt.* and $\frac{1}{2}$, and the black at 1 *oz.* The scale, with the silk lines that belonged to it, and the hook, was adjusted precisely to the weight of 1 *oz.*; and as I commonly measured the strength of cohesion by fixing the hook to the black stocking, and taking hold of the white, I had but to make an allowance of 2 *oz.* more than the weights put into the scale, so as to take the precise weight the stockings could raise by the power of cohesion.

I measured this power two different ways; the first whilst the one stocking was still within the other; secondly, when separated, and the one afterwards applied externally to the other. In the first of these cases, it may be thought that an allowance should be made for the friction in pulling the stockings asunder; but that appeared to me to be very inconsiderable; for
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when those of the same colour were put one within the other, and inverted, they dropped afunder of themselves; or if there was any intanglement about the heel, a little shaking difengaged and feparated them: however, if it should be thought proper, the allowance of an ounce may be made, by deducting fo much from the weight refpectively found.

I have but one circumftance more to mention, before I proceed to give the refult of my experiments; which is, that we muft not be furprifed, when we find the force of cohefion externally, to be in no regular proportion with that within; for when the ftockings are highly electrified, they ruft together with fuch impetuofity, that it is extremely difficult to direct their motion, and make them meet in the manner moft advantageous for cohefion.

In the experiments I have made, to meafure the force of electrical cohefion, I have always found it anfwerable, fo far as I could judge from appearances, to the degree of electricity at the time excited. When the ftockings have been but weakly electrified, I have found them unable to fupport the weight, the one of the other. When in a more powerful ftate of electricity, I have known them to raife, refpectively, from one to twelve ounces, and upwards; nay, once I found the cohefion fo ftrong as to move feventeen ounces, including the fcale and the black ftocking. For the fake of accuracy, I fhall give a particular account of the refult of a few of my moft remarkable experiments, as I find them in the notes, which I took at the time of making them.

• The firft I find is of the 30th of March; the wind at north-eaft; the weather clear, inclining to

frost. The white stocking within the black, raised the weight of 1 *lb.* 1 *oz.* 6 *dwt.* $\frac{1}{2}$, half a pennyweight more separated them. I find no note here taken of the weight born by the stockings applied externally to each other.

April 6. A hazy and wet morning; but the wind at north-east. With new stockings; the white, being within the black, raised 11 *oz.*—without, 6 *oz.* With another pair, that had been much used in experiments; the white, within, raised 6 *oz.*—without, 3 *oz.*

April 16. A clear dry morning; the wind at north-east; Fahrenheit's thermometer at 48. Of the new, the white stocking, within, raised 1 *lb.* 5 *oz.*—without, 6 *oz.* 15 *dwt.*—Of the old, the white stocking, within, raised 8 *oz.*—without, 5 *oz.* 15 *dwt.*—

The last observation I find marked, is of the 19th of May; the weather clear; the wind at north-east; the thermometer at 55. The white, within the black, raised 10 *oz.*—and without, 6 *oz.*

It is to be remarked, that by this time I had got the old stockings washed; and now there appeared little or no difference between the power of them and of the newer pairs; though the latter had still the advantage.

In making the experiments, it was necessary to lose as little time as possible, on account of the waste of electricity: I could therefore seldom proceed to the accuracy of fractions of a penny weight; nor often indeed of those of an ounce; and this is the reason that my notes run generally in round numbers, which, however, may be depended upon.

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The greatest weight I have been able to raise by the force of electrical cohesion, as appears above, has been seventeen ounces. Now the white stocking, which weighed but 17 *dwt.* and an half, bore all this weight: in this case therefore it raised, by the strength of its cohesion with the black, 340 pennyweight; that is, nearly twenty times its own weight*. And if we consider that the force, applied to separate them, acted in a direction parallel to the surfaces, by which they cohered; and that when the surfaces are smooth, a force acting in such direction, has much greater influence in separating bodies, by making them slide gently over one another, than if those bodies were rigid, and the force employed to separate them acted in a direction perpendicular to the cohering surfaces;

* As the experiments mentioned above were made in the space of six or seven weeks from about the latter end of March, when the temperature of the air begins to be less favourable for electricity, I did not doubt, but upon the approach, and during the progress of the ensuing winter, I should meet with instances of a stronger degree of cohesion, than I had before been able to ascertain. Accordingly, since this paper was read in the Royal Society, and particularly in the months of November, December, and January last, at times, when the weather was clear and frosty, I found that the same stockings lifted considerably more, than I had been able to make them do in the preceding months of March, April, and May. I likewise found, that when the stockings were perfectly new, or the black dyed afresh, and the white newly cleaned and sulphured, as also, that when they were of a more substantial make, such as those that are wove of spun silk, weighing commonly about the double of those that go by the name of half gauze, their power of cohesion, when favoured by the temperature of the air, increases to a very considerable degree. Under those circumstances, at particular times, I have been able to make the black stocking, or the white, when the rough sides of each were put together, raise (the half gauze) from 20 to 40, and (of spun silk) from 40 to 90 times its own weight. Vide the subsequent letter from Dr. John Mitchell to the Rev. Dr. Birch.

when we consider this, I say, it will be hard to determine how great the strength of their cohesion may be.

The experiment may be tried with two plates of glass, the one electrified *positively*, the other *negatively*. As in that case the principle, upon which the cohesion of the white with the black silk depends, would take place, I make no manner of doubt but the effect would be the same. I have not had an opportunity of trying the experiment: but I should expect that the two plates would be found to cohere with greater force, than I have been able to ascertain in the stockings; as a contrivance might be made to pull them asunder in a direction perpendicular to their cohering surfaces.

The force with which the black and the white stocking cohere, is not the only thing remarkable in their junction. The solution of that cohesion, and the different degrees of tenacity, according to different circumstances, afford some curious observations.

When the black and the white stocking are in cohesion with each other, if another pair, more highly electrified, be separated, and presented to the former still in conjunction, the black to the white, and the white to the black; in that case, the cohesion of the first pair will be dissolved, and each stocking of the second, will carry off that of its opposite colour adhering to it. If the degree of electricity of both pairs be equal, the cohesion of the first pair will be weakened, but not dissolved; and all the four will cohere, forming as it were one mass. If the second pair be but weakly electrified, the cohesion of the first pair with one another will be but little impaired,

ed, and that of the stockings of the second with those of the first, will be weak in proportion. And lastly, if the second pair be not at all electrified, or if, in their place, any other body not electrified be presented, there will be no effect produced on either hand.

White silk and black, when electrified, not only cohere with each other in the manner shewn above, but when in a high degree of electricity, are found, both one and the other, to adhere to bodies of broad and even, or polished surfaces, though those bodies be not electrified. This adhesion I discovered accidentally. While I was about some electrical experiments, having, without design, thrown a stocking, that was highly electrified, hastily out of my hand, I was surprised to find it some time after, sticking against the paper-hangings of my room. This led me to make the following experiments.

I presented the white and the black silk, highly electrified, and in cohesion with each other, to the hangings; but no effect was produced. I then separated the black from the white, and presented them singly; in that case each of them readily adhered to the hangings, which they likewise did when flung from a little distance, and continued there for near an hour before they dropped. Having stuck up the black and the white, in the manner above-mentioned, I came with another pair of stockings highly electrified, and applying the white to the black, and the black to the white, I carried them off from the wall hanging on those that had been applied to them. When the second pair were electrified, but to a moderate degree, on applying them, in the manner above described,

described, the former immediately quitted their hold of the hangings, and dropped to the ground. The same experiments held with the painted boards of the room; and likewise with the looking-glasses; to the last of which, both the black and the white silk, appeared to adhere more tenaciously, than to either of the former.

I am afraid I have presumed too much on the patience of the Society, by giving so full a detail of my experiments and observations, on a branch of electricity, that takes its rise from so singular, and so mean a subject as that of black and white silk: But however particular the subject may be, the properties of electricity that are thence experimentally deduced are of a general nature, and must find a place in every true system of electricity. If any apology be wanted, the best I can make, is to endeavour to draw such inferences from the experiments above recited, as may possibly throw new light upon the theory of electricity.

But lest I should swell this paper to too large a size; and as the season is at hand when the Society usually adjourns for some months; I think it best to defer any matter I may have to offer, concerning the theory of electricity, till a convenient time after our next meeting. I shall only beg leave at present, to mention one or two things, which I have not hitherto had a proper opportunity of throwing out.

It hath been said, that the influence of colours in electricity is no new discovery; that Monf. Du Fay had treated at large upon the subject; and, after a variety of experiments, had concluded, that colours, as such, had no effect in electricity; but that whatever

was

was remarkable in that way, was owing to the change, which the ingredients of the dye produced upon the coloured body. I had not then seen what *Monf. Du Fay* wrote upon the subject; but as I have since, I shall take the liberty to state this matter fairly.

The late *Monf. Du Fay*, an ingenious member of the Academy of Sciences at Paris, to whom we owe some valuable discoveries in electricity, gives an account of what is here alluded to, in a memoir, presented in the year 1733. Electricity was at that time in its infancy; *Mr. Hawksbee* had, but a little before, published an account of his experiments; which brought such surprising appearances of electricity to light, as could not but induce the curious to turn their eyes upon that subject. In the course of those experiments, he had taken notice of something remarkable with regard to colours. *Mr. Gray* succeeded, and having opened a new path, made still further discoveries in electricity: he likewise, in giving an account of what he had observed, hinted at something curious with regard to colours. But neither of them appear to have come to any determined point in this matter. *Monf. Du Fay*, who concurred with *Mr. Gray*, in carrying on electrical discoveries, with a candour and ingenuity that did honour to them both, having entered upon an enquiry (the subject of the memoir above-mentioned) to determine what sort of bodies were most susceptible of electricity, thought proper, in consequence of what had fallen from *Mr. Hawksbee* and *Mr. Gray*, to examine what effect the different colours had in augmenting or diminishing the electricity of different substances.

Accordingly

Accordingly he ranged a number of ribbands, of all the primitive colours, hanging them in the same vertical plane; and to these he applied an excited glass tube, in an horizontal direction.—Upon this he observed, that the black was first attracted; and, as he brought the tube nearer, the white next; and the rest successively, though not always in the same order. He made another experiment, in the same view, with gauzes of different colours, through which he tried the force of an excited tube, upon light bodies placed at a proper distance behind them: and from the result he was of opinion, there was something in the influence of colours. But having afterwards tried some experiments with the coloured rays of the sun as refracted by a prism, with flowers of different colours, and with white ribbands rubbed over with differently coloured substances, he began to change his opinion. He likewise had recourse to what he calls a decisive experiment: he dipped his different-coloured ribbands in water; and when they were all equally wetted, he applied his tube, and found they were all equally attracted. From this last-mentioned experiment, in particular, he concluded that colours, as colours, had no effect in electricity; but that all was owing to the ingredients of the dye imbibed by the coloured body.

It is not my purpose here to inquire, whether *Monf. Du Fay's* conclusion is well or ill founded. Whatever may be the decision of that point, I apprehend the whole of this affair hath very little concern with what hath been the subject of these papers, and could have been of little use to me, had I been acquainted with it before.

The

The series of my experiments, however inconsiderable they may prove in the result, have taken a turn I did not at first foresee. I set out with inquiring into the nature of an electricity, that seemed to have some connexion with the human body: I had made but little progress when I was surpris'd with an appearance of electricity, arising from the contrast of two colours, or coloured bodies of the same substance: I met with it in my experiments, perpetually *positive* and *negative*; and under that appearance have followed it through a variety of its operations. The notions it hath led me to conceive, are different from those I had before entertained of electricity. Such as they are, they shall, in due time, be freely submitted to the judgment of the Society.

Having been told by one or two of my friends, that they had tried some of the experiments mentioned in the preceding papers, but could not get them to succeed; I beg leave to add a few words, before I conclude, by way of caution to any, who may have a curiosity to verify my experiments.

However easy it may seem to be, to follow the directions I have already given in electrifying the stocking, I am sensible from my own experience, that an attention to a number of little circumstances, besides some small degree of address, is requisite, in order to make it succeed readily. This is known to have been the case with the electrical tube and globe: few people, at first, knew how to manage them successfully; and yet glass is not so much exposed to many inconveniencies, that affect electricity, as silk. To give but one instance of this: we know that a very small quantity of dust, grease, or any other matter that

sticks to the tube, will injure its electricity: Now this is what may easily be wiped off from glass; but it may be very difficult to clear the silk of it, as silk is of a nature more apt to imbibe, and to retain extraneous matter than glass.—From these considerations, I should not be surpris'd at any persons being disappointed, as to the success of their experiments, in a few trials; but I should think it not fair for them to conclude from thence, that those above-recited are not to be depended upon *. Yet, if any member of the Society, who is curious in these matters, hath not been able to satisfy himself with regard to any particular experiment, I shall think it no trouble to shew him, at any proper time, how it succeeds with me.

* The circumstances necessary to be attended to, in order to procure the most considerable appearance of electricity, are as follow. 1°. That the wind be in the north, north-east, or east; the weather clear and frosty, or inclining to frost. 2°. That the stockings be of silk; one of a good black, the other of a clear white; and both of the same size and substance. 3°. That they be new; or be kept carefully wrapt up in paper, and only used in electrical experiments. 4°. And lastly, That they be dried before the fire, and warmed to about the degree of heat of the human body, before they be drawn upon the hand or leg. And when taken off, care should be had in separating them, so as to keep them at a distance from one's clothes, or any thing that may lessen their virtue. The circumstances above-mentioned, might be gathered from what has been said in these papers; but I thought it not amiss to bring them together in this note.

*New Experiments and Observations concerning
Electricity.*

P A P E R IV. P A R T I.

Of Two distinct Powers in Electricity.

Read Dec. 20, 1759. **I** Observed, in the papers I have already had the honour of communicating to the Society, that the different state of electricity, as distinguished into *positive* and *negative*, was a leading circumstance in all electrical appearances; and that, if followed out with due attention, it might bring us to a clearer view, than we yet have had, of the powers that nature employs in those wonderful operations. Nothing could be more proper to serve this purpose, than the simple apparatus I had all along made use of. It consisted of two electric bodies, of which, when they were excited, one never failed to assume the positive, the other the negative state of electricity. The observations I had frequent opportunities of making, with regard to the manner in which they acted, in these different states, on other bodies, and on each other reciprocally, confirmed me in a notion, which, in the course of my experiments, I had very early conceived.

My notion is, that the operations of electricity do not depend upon one single positive power, according to the opinion generally received; but upon two distinct, positive, and active powers, which, by ~~crafting~~ contrasting, and, as it were, counteracting each other,

produce the various phenomena of electricity; and that, when a body is said to be positively electrified, it is not simply that it is possessed of a larger share of electric matter than in a natural state; nor, when it is said to be negatively electrified, of a less; but that, in the former case, it is possessed of a larger portion of one of those active powers, and in the latter, of a larger portion of the other; while a body, in its natural state, remains unelectrified, from an equal ballance of those two powers within it.

I do not here undertake to give a system of electricity. It is the design of this paper to propose the grounds of my opinion, only so far as it rests upon observation and experiment. If the proofs I have to offer shall be found conclusive, and it be allowed, that two distinct and counteracting powers prevail in electricity, one of them corresponding with the positive, and the other with the negative state; in that case, the truth thus established, may afterwards be assumed as a principle in theory; and we may try how far it will serve in accounting for the various appearances of electricity. But even then, I should not think of troubling the Society with discussions of so great a length, and of so speculative a nature, as those usually are that relate to theory.

I might bring arguments to prove the existence of two distinct powers in electricity, from a variety of observations and experiments, some of them taken from among those mentioned in the preceding papers: But as the bounds I prescribe to myself in this, do not permit me to enter upon so large a field, I shall confine myself to such observations only, as have fallen within

within my notice, in one particular branch of electricity, that which relates to the Leyden phial.

All who allow of *positive* and *negative* electricity, know, that the phial, when charged, exhibits electricity in those two states, the one within, the other on the outside; and that when a communication is made between the two, by the means of a non-electric touching the coating, and at the same time approaching the wire, or *vice versa*, the explosion is produced, and the phial discharged. This reduces the question to a narrow compass; for if, upon the discharge of the phial, we meet with proofs not only of a power acting from within to the outside, but also of a power acting at the same instant from the outside to within, then, I think, we may fairly conclude, that what is called *negative* electricity is, in reality, a positive active power; and that electricity, in general, consists not of one alone, but of two distinct, positive powers, acting in contrary directions, and towards each other.

The proof I shall offer first, is founded on the following experiment. When the phial is electrified but a little, if we touch the coating of it with a finger of one hand, and at the same time approach a finger of the other hand to the wire, we shall receive a pretty smart blow upon the tip of each of the fingers, the sensation of which reaches no farther: If the phial be electrified a degree higher, we shall feel a stronger blow, reaching to the wrists, but no farther: When again it is electrified to a still higher degree, a severer blow will be received; but will not be felt beyond the elbows: Lastly, when the phial is strongly charged, the stroke may be perceived in the wrists and elbows;
but

but the principal shock is felt in the breast, as if a blow from each side met there. This plain and simple experiment seems obviously to suggest to observation, the existence of two distinct powers, acting in contrary directions : And I believe it would be held as a sufficient proof by any who should try the experiment, with a view to determine the question simply from their own perceptions.

But as I am sensible, that the proof of any important point in philosophy, ought not to depend upon the perceptions of this or that particular person, I judged it necessary to have recourse to experiments, the result of which might admit of no ambiguity. The fortunate discovery of M. Muschenbroek and M. Allamand, with the improvements that have since been made upon it, puts it in our power to increase electricity to what degree we please. I did not therefore despair of the means of bringing this matter to a fair decision. I expected, that if an electrical stroke should be made to pass through a solid body, with so much force as to pierce and tear the substance of it, such marks would be left, as might enable us, with certainty, to trace the course of the electrical power in its passage through the body.

Having no apparatus of my own capable of producing such effects, I had recourse to a worthy member of this Society, doctor Franklin, who was possessed of a very good one. I had communicated all my observations to this gentleman as they occurred, and, in return, met with an ingenuity and candour, that render him as estimable in private life, as the improvements he has introduced into electricity, and particularly his discovery in relation to thunder-
and

and lightning, will render his reputation lasting in the learned world. We differed in opinion with regard to the point in question; nevertheless I found him ready to give me all the assistance in his power, for bringing the matter to a fair decision. I had seen him pierce a quire of paper with a stroke of electricity; and as I perceived it had been struck several times before, I desired he would give it me, that I might at leisure examine the effects of the sundry strokes.

When I came to do so, I observed, that at every hole which had been made through the quire, the upper and the under leaf (for the quire had been laid in an horizontal position when it was struck) were ragged about the orifice, and those ragged edges pointed mostly outwards from the body of the quire. But what was more material; when I came to turn over the leaves, I found, that the edges of the holes were bent regularly two different ways (and more remarkably so about the middle of the quire), one part of each hole upwards, and the other part downwards; so that, tracing any particular hole as it traversed the quire, I found on one side the fibres pointed one way, and on the other side the other way; much in such a manner, as if the hole had been made in the quire, by drawing two threads in contrary directions through it.

This was not all: A piece of paper, covered on one side with Dutch gilding, had been accidentally left between two leaves in the quire, and had been pierced by two different strokes. This exhibited a very remarkable appearance: Where each of the strokes had been given, the gold leaf was stripped off, and had
left

left the paper bare for a little space, in an oblong form, rounded at the ends; in which, at the distance of about a quarter of an inch from each other, appeared two points, one of them a little round hole, the other only an indent or impression, such as might have been made by the point of a bodkin. In the leaf, which fronted the gilding, two such points likewise appeared, corresponding to those above-mentioned; so that the hole in the one was opposite to the impression in the other, but surrounded with little black or blueish circles. When the hole, which had been struck in the quire, was traced from above down to the gilding (for the gilt paper happened to lie with its gilded side uppermost), it was found to terminate on the point in the gilt paper where the impression appeared, and there the impression pointed downwards. Again, when the hole in the lower part of the quire was traced from below upwards, it was found to terminate on the point in the leaf fronting the gilding, where the impression was, and there the impression pointed upwards. The facts above-mentioned seem to leave it without doubt, that the stroke had been given, at the same instant, upwards and downwards; but that the electrical power from above, and from below, had seized upon the gilding, dissipated part of it in vapour, and by that means become so weak, that each of them could afterwards only make an impression upon the paper, marking the respective directions of their course.

I communicated these observations to Dr. Franklin; but as no conclusion can, with certainty, be drawn but from facts, confirmed by repeated trials, I desired to have the satisfaction of making a few experiments

riments with him in relation to this matter ; to which he readily consented. For that purpose I waited upon him one morning about the middle of June last ; and the better to ascertain what was essential in the facts, I varied the circumstances a little from those above.

In the middle of a paper-book of the thickness of a quire, I put a slip of tin-foil ; and in another of the same thickness I put two slips of the same sort of foil, including the two middle leaves of the book between them. Upon striking the two different books, the effects were answerable to what I expected. In the first, the leaves on each side of the foil were pierced, while the foil itself remained unpierced ; but, at the same time, I could perceive an impression had been made on each of its surfaces, at a little distance one from another ; and such impressions were still more visible upon the paper, and might be traced as pointing different ways. In the second, all the leaves of the book were pierced, excepting the two that were between the slips of foil ; and in these two, instead of holes, the two impressions, in contrary directions, were very visible.

I have lately got an electrical apparatus of my own, formed on the model of that of Dr. Franklin's, and have had opportunity since, of making frequent repetitions of the experiments above-mentioned. Notwithstanding some little variation in appearances, arising, as in other electrical experiments, from the particular state of the weather, the different degree of electricity, or other accidental circumstances, I have met with nothing but what confirms me in my opinion of two distinct counteracting powers. All

the remarks I have been able to make in the repetition of experiments, that need to be added to what I had before observed, may be reduced to the three following.

1°. When a quire of paper, without any thing between the leaves, is pierced with a stroke of electricity, the two different powers keep in the same tract, and make but one hole in their passage through the paper: not but that the power from above, or that from below, sometimes darts into the paper at two or more fundry points, making so many holes, which, however, generally unite before they go through the paper. What I mean is, that I never yet could observe the two powers to make different holes in the paper; but that they always keep the same common channel, rushing along it with inconceivable impetuosity, and in contrary directions. They seem to pass each other much about the middle of the quire; for there the edges are most visibly bent different ways: whereas in the leaves near the outside of the quire, the holes very often carry more the appearance of the passage of a power issuing out, and exploding into the air, than of one darting into the paper.

2°. When any thin metallic substance, such as gilt-leaf, or tin-foil, is put between the leaves of the quire, and the whole struck; in that case, the counteracting powers deviate from the directest tract, and leaving the path they would in common have taken through the paper only, make their way in different lines to the metallic body, and strike it in two different points, distant from one another about a quarter of an inch, more or less (the distance appearing to be least when the power is greatest;) and whether they
 pierce,

pierce, or only make impressions upon it, in either case, they leave evident marks of motion from two different parts, and in two contrary directions. It is this deviation from a common course, and the separation of the lines of direction consequent upon it, that affords us the strongest proof, of the exertion of two distinct and counteracting powers.

3°. When two slips of tin-foil are put into the middle of the quire, including two or more leaves between them, if the electricity be moderately strong, the counteracting powers only strike against the slips, and leave their impressions there. When it is stronger, we generally find one of the slips pierced; but seldom both: and from what I have observed in such cases, it would seem as if the power, which issued from the outside of the phial, acts more strongly than that which proceeds from within; for the lower slip is most commonly pierced: But that may be owing to the greater space, the power from within has to move through, before it strikes the paper.

I take the liberty to lay before the Society a paper-book, of the thickness of a quire, struck three times in the manner above described. The first stroke (A) is given, when there is nothing between the leaves of the book. The second (B) when a piece of paper, covered on one side with what is commonly called Dutch gilding, is laid in the middle. The third (C), when two slips of tin-foil are put into the book, including the two middle leaves between them.

The members, who are curious in these matters, may, at their leisure, examine the effects of the sundry strokes; and if any gentlemen, in particular, desire the farther satisfaction of seeing the strokes given,

I shall be very ready, at any proper time, to comply with their desire.

New Experiments and Observations concerning Electricity; by Robert Symmer, Esq; F. R. S.

P A P E R IV. P A R T II.

Of Two distinct Powers in Electricity.

Read Dec. 20, 1759. **T**HE notion of two distinct electrical

powers, acting in contrary directions, may appear to some to be the same with that of the *effluence* and *affluence* of electrical matter, which M. l'Abbé Nollet gives as the general cause of the phenomena of electricity *. It may therefore be not improper to take a nearer view of these two opinions, to see how far they agree, and in what they differ.

This ingenious author, whose merit in the learned world is very considerable, particularly with regard to his labours in electricity, had observed, that, when a body is electrified, a current of electric fluid issues from it, and, in the form of diverging rays, spreads through the air, and enters into other bodies; and that, at the same time, a current of electric fluid,

* “ Plus de trois ans se sont écoulés depuis que j'ai proposé
 “ comme la cause generale des phénomènes électriques, l'*effluence*
 “ et l'*affluence* simultanées d'une matiere fluide, presente par tout, et
 “ capable de s'enflammer par le choc de ses propres rayons.” Preface
 to *Recherches sur l'électricité*, at the beginning.

issuing from other bodies, passes through the air, and, in the form of converging rays, enters into the body electrified. From thence he concludes, that a continued, and (to use his own terms) simultaneous * effluence and affluence of a fluid matter, extremely subtile, constitutes † electricity. Upon this principle he endeavours to account for all the phenomena that attend the electrification of bodies.

What M. l'Abbé Nollet has observed with regard to two contrary currents in electricity, is by no means inconsistent with the principle of two distinct counter-acting powers. On the contrary, the existence of two such currents is, according to my opinion, a necessary consequence of the exertion of those powers from one body upon another. It is a phenomenon of electricity only; not the principle upon which all electrical appearances depend.

But a more essential difference takes place between this gentleman's opinion and mine: he represents the two currents as consisting but of one and the same

* “ Ces deux courans qui ont des mouvemens opposés, ont lieu tous deux ensemble, c'est ce que j'exprime par le mot simultanés.” *Lettres sur l'électricité*, p. 30.

† “ L'électricité, comme je l'ai déjà dit et prouvé ailleurs, n'est pas seulement l'émanation d'une matière qui s'élance du corps électrisé; c'est aussi un remplacement continu qui se fait de cette matière, par une autre tout-à-fait semblable, qui se porte de toutes parts au corps électrisé; c'est pour ainsi dire, un commerce de la matière qui j'ai nommée effluente, et de celle que j'ai appelée affluente. Si celle-ci vient à manquer, ou que la première n'ait plus la liberté de sortir, cet état ou ce double mouvement, que l'on nomme *électricité*, doit bien-tôt cesser.” *Essai sur l'électricité*, p. 202.

fluid ‡; admits but of one kind of electricity ||; and maintains, that two bodies cannot be said to be differently electrified, but as they are electrified in a higher or lower degree *. On the other hand, it is my opinion, that there are two electrical fluids (or emanations of two distinct electrical powers) essentially different from each other; that electricity does not consist in the efflux and afflux of those fluids, but in the accumulation of the one or the other in the body electrified; or, in other words, it consists in the possession of a larger portion of the one or of the other power, than is requisite to maintain an even ballance within the body; and, lastly, that according as the one or the other power prevails, the body is electrified in one or in another manner.

In those respects we differ in opinion. Who is in the right is another question. The whole seems to turn on a single point, namely, whether there be but one, or if there are two distinct kinds of electricity. The bounds of this paper do not permit me to enter upon a full discussion of the point. I cannot, however, but observe, that the whole series of experiments mentioned in the preceding papers, tends to confirm the distinction, formerly made, of electricity into two kinds; and to shew, that there is an essential difference (whatever it be that constitutes that difference) between what is commonly called positive electricity, and negative. A farther proof of that difference arises from the success of an experiment, of

‡ *Essai sur l'électricité*, p. 160, 161.

|| *Ibid.* p. 118, 119.

* *Lettres sur l'électricité*, p. 101. 105.

which I threw out a hint in my third paper, and which I have since taken an opportunity of making, touching the electrical cohesion of glass. The experiment is as follows :

I took two panes of common window-glass, about nine inches square, the thinnest, the most even, and the smoothest in their surfaces I could get. I covered one of the sides of each with tin-foil, leaving the space of near an inch from the edges uncovered. I warmed them a little at the fire ; and, applying the two bare sides together, I laid them upon four wine-glasses, which supported them at the corners. I then brought down a chain from the prime conductor, nearly to touch the coating of the upper plate, and applying a wire, which I held in my hand, to the coating of the under plate, the machine was put in motion, and the electrification performed, as in the case of the common electrical pane. When the operation was completed, I removed the chain and the wire, and taking hold of two opposite corners of the upper glass (those corresponding to them in the other having been purposely cut away, I lifted it, and found, that the under glass came up with it. The cohesion appeared to me to be considerably strong ; but I had not any proper apparatus ready to measure the strength of it. I laid them down again on the wine-glasses, and procured an explosion, as in the case of the common electrical pane. I then took hold of the corners of the upper glass, and lifted it ; but found, that the cohesion was dissolved, the under glass remaining behind.

I could indeed perceive, that, after the discharge, there was still some small degree of cohesion between the plates, which felt as if some glutinous substance
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had got feeble hold of them : but this was no more than what I found took place between them, when, without being electrified, they were forced close together. For that reason, two plates of glass, finely polished, and so even as to come into close contact through the whole extent of their opposed surfaces, would be very improper for this experiment ; for, when the power of electricity had forced them into contact, the pressure of the air, and a cohesion proceeding from another principle, would keep them together.

But to pursue the purpose of our experiment.—All who admit of the distinction of electricity into two kinds, agree, that as in the Leyden phial, so likewise in the electrical pane, the different sides are differently electrified : That side, which more immediately receives its electricity from the glass globe, is said to be positively, and the other negatively, electrified. What may be said of the electrical pane, is applicable to the glass plates in this experiment ; for, when they are put together in the manner mentioned above, they form an electrical pane between them ; one of the plates corresponding with one of the sides, and the other with the other side of the pane. When, therefore, the glass plates are electrified in the manner before described, the plate, which receives its electricity immediately from the chain, will, according to this distinction, be positively electrified, and that which receives its electricity from the wire, negatively.

Upon these considerations, we may expect, from the experiment in hand, the means of determining, whether the distinction of electricity into two different kinds is merely nominal, or if there is an essential difference

difference between them : For after the glass plates have been electrified in one position, so as to be incapable of receiving any more electricity, if they be inverted, and in that new position presented to the chain and wire, and the globe again be put in motion, according as one or other of those opinions holds, correspondent effects will follow. If the electricity, that comes by the chain, be of the same nature with that which comes by the wire, no change will be produced upon the plates ; for being before full of one and the same kind of electricity, they can do no more than keep what they had, or exchange it for just as much of the same kind. But if, by the chain and the wire, two kinds of electricity, totally different in their nature, be conveyed into the respective plates, in that case it is to be expected, that the electricity that each of them had acquired in their former position, will be gradually destroyed, till no signs of electricity appear in either ; after which, they will begin again to be electrified, having their electricity reversed.

In order to see what would really happen, I repeated the experiment in the following manner : I electrified the two plates till they were fully charged, and in strong cohesion, the snapping from the chain and the wire having totally ceased. I then turned them upon the glasses that supported them, applied the chain and the wire to the different sides, and began to electrify as before. The glass globe was no sooner in motion, than the snapping from the chain and the wire returned with violence ; and the plates, which, in the former position, would receive no more electricity, appeared, in their new situation, to receive

it both from above and below, more greedily than ever. As this new electrification proceeded, I found, by several trials, that the cohesion became gradually weaker, till, at a certain period, it was totally dissolved; from which, it began again to be restored, and at last, when the snapping ceased, I found it to be as strong as before.

I carried the experiment still farther: I took two complete electrical panes (that is, two glass plates, covered each on both sides with tin-foil), and laying one upon the other, I applied the chain and the wire, and proceeded to electrify. The electrification took place throughout; and I could procure an explosion from either of them single, or from both together: but however highly they were electrified, I never could perceive the least appearance of cohesion between them. This was agreeable to what I expected. I judged, that, in consequence of two different kinds of electricity, each of the panes would be charged, on its different sides, with the different kinds; which, by counteracting one another in the same pane, would reduce it to a neutral state of electricity, and by that means prevent the two panes from acting on each other.

It is not here my purpose to account for electrical cohesion; yet I cannot but observe, that, in this case at least, it is obvious, that the cohesion cannot be owing to an *effluence* and *affluence* of one and the same electrical fluid: For the two plates being of the same substance, and in every respect alike, the effluent current must have just as great an effect in separating them, as the affluent can have in bringing or keeping them together. The experiment above seems to
make

make it evident, that there are two distinct kinds of electricity; and the influence of those in making the plates cohere, seems naturally to denote, in concurrence with the experiments mentioned in the former part of this paper, two distinct and counteracting powers, corresponding with the two different kinds of electricity.

If, upon the whole, the arguments I have brought to prove the existence of two distinct powers in electricity, are found to be conclusive, it may, perhaps, be expected of me to say something of the nature of those powers. Without entering into any particular theory, or indulging myself in loose conjecture, I shall take the liberty to offer a few considerations, such as occur to me on this occasion.

All we know of active powers, extends no farther than as we perceive them to be effects of a power still more general; or as we find them producing effects according to certain laws. I have not been able to trace the powers of electricity farther back, than the observations I have given above, have led me. I do not therefore take upon me to determine, whether they consist of the finer parts of matter, constituting an active and elastic fluid, the elasticity of that fluid remaining still to be accounted for; or if they are of a substance yet more subtle and active, of which, however, we have hitherto been able to form no distinct idea. Whatever other power they may be the immediate effects of, or whatever be the secret and imperceptible manner in which they act, the more interesting object of our inquiry, is to know the laws according to which they act, and how far their operations extend in the material world.

The same observations that lead us to the discovery of any power, if followed out with due attention, may serve to instruct us in the laws of its action; and if we can attain a sufficient knowledge of those laws, however ignorant we may be of the peculiar manner in which the power exerts itself, we may be, by that means, enabled to trace it through its various operations.

The laws of nature are few and simple. It is only from the variety of circumstances, under which the respective powers are exerted, that the phenomena of nature are multiplied. The powers of mechanic motion, those I mean by which bodies act upon one another in impulse or pressure, are found to be under the direction but of three general laws; and from thence is deduced a most extensive branch of natural knowledge.

The laws that regulate the powers, by which bodies act at a distance upon one another, when we come to be better acquainted with them, may be found to be not more numerous or complex. What appears wonderful to us, is, that bodies should at all be capable of acting upon one another at a distance: But are we not equally ignorant of the manner, in which the powers of motion are exerted, when the bodies are in actual congress? Daily experience convinces us of the fact in this case; and in the other, repeated experiments, and frequent observations, leave us little room to doubt, that there are powers, which, when lodged in one body, are capable of being exerted upon another at a distance.

The powers of electricity are found to be of this nature. When either of those powers prevails in a
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body,

body, it exerts its influence, so long as it is lodged there, every way around, and, by pressure or otherwise, acts upon another body within the sphere of its influence. If it meets with no resistance, it enters this other body, and from this begins to act, as it did from the former. In the mean while, the counteracting power prevailing in this other body, the like effects will be produced: and thus these two bodies, by means of their corresponding powers, will continue to act upon each other, till their powers be reduced to an equal ballance.

Did electricity consist but of one power, after an experimental inquiry into the laws according to which it acted, we might, upon that principle, be able to account for the phenomena of electricity. But if two distinct and counteracting powers prevail, as appears to me to be evident from the preceding experiments and observations, in that case, it will be impossible to give a complete and consistent theory of electricity, but upon the principle of two such powers. Nor will this principle be found, upon due consideration, to disagree with the general system of nature. It is one of the fundamental laws of nature, that action and re-action are inseparable and equal. And, when we look around, we find that every power, that is exerted in the material world, meets with a counteracting power, that controls and regulates its effects, so as to answer the wise purposes of Providence.

A Letter to the Reverend Dr. Birch, Secretary to the Royal Society, concerning the Force of electrical Cohesion.

S I R,

Read Dec. 20. 1759. **I** Happened to be at Mr. Symmer's on Saturday the 15th instant, when he desired me to be witness to some electrical experiments, he was about to make, with silk stockings, of a particular kind, which he had received for that purpose.

The weather was then remarkably favourable for electricity, being clear and dry, with a sharp frost, which had continued five or six days. The wind was easterly, and had been in that quarter for ten days. It was about noon when we made our experiments; the barometer at 30, and Fahrenheit's thermometer at 32.

The stockings above-mentioned were wove of carded and spun silk, and were more substantial and weighty, than those with which he had made the experiments mentioned in his third paper. One pair was of a deep black, having been twice dyed, in order to improve the colour. Another pair was of the natural colour of the silk, of a dusky white; and both new. The pair of black weighed four ounces, eight pennyweight, and four grains; and the white three ounces, eighteen pennyweight, and fifteen grains.

We began with making a few experiments with the thin stockings formerly made use of; and found the result to be much the same with what is related

by Mr. Symmer in his third paper : that is, we found, that when the white stocking was put within the black, or *vice versa*, and both highly electrified, taking hold of the one, while a scale with weights was put to the other, we could raise seventeen ounces before the stockings separated.

We then repeated one or two of those experiments with some little variation of circumstances. We turned one of the stockings inside out, and put that within the other : the inner or rough sides of the stockings being thus together, by which means they took faster hold of each other, we now found, that it required the weight of twenty ounces to separate them.

When the stockings were separated, and applied externally to each other, they then raised the weight of ten ounces *.

We

* Some time after this letter of Dr. Mitchell's had been read in the Society, as I was of opinion, that the thin stockings, mentioned above, had lost much of their electrical power since the beginning of April last, when they first had been made use of in experiments, and that it was owing to the peculiar influence of the weather, that they raised as great a weight now as they had done then, I had the black new dyed, and the white washed, and afterwards whitened in the fumes of sulphur. Upon this, I found their force very much increased. On the 9th of January, the weather being much in the same state as it had been the 15th of the preceding month, the stockings, thus prepared, and put one within another, having their rough sides together, lifted no less than three pounds and three ounces before they separated. Dr. Mitchell was likewise present at this experiment.

How far the circumstance of smoking the white stocking in the fumes of sulphur, might have contributed to increase the electrical power, is what I cannot take upon me to say. I should think, however, that it does not much contribute to it ; for we find, by the experiments in the sequel of this letter, that the force of cohesion

We next proceeded to try the force of electrical cohesion, with the stockings of a more substantial make; viz. those I have above described; and there we found it to be much more considerable, as appears by the following experiments.

1°. When the white stocking was put within the black (without either of them being turned inside out), so that the outside of the white was contiguous to the inside of the black, they lifted nine pounds, wanting a few pennyweight. Now, taking the weight of the stocking to be one ounce, eighteen pennyweight, and fifteen grains (viz. the half of the weight of the pair as mentioned above), it follows, that, by the force of its cohesion with the black, it raised fifty-five times its own weight.

2°. When the white was turned inside out, and put within the black, their inner or rough sides being contiguous, they lifted no less than fifteen pounds, one pennyweight and a half, before they separated: So that, in this case, the single stocking raised ninety-two times its own weight †.

fion is surprisngly great between the black and the white stocking of spun silk, when electrified: and yet I was assured by the hosier, that the white had never been put into the fumes of sulphur; and that the colour it had was the natural colour of the silk, no other method having been taken to whiten it, than that of scouring and washing.

† Since that time, I have not been able to raise above ten or eleven pounds with these stockings, even when the weather has been most favourable; owing, perhaps, to my having cut off all the ends of threads, and tufts of silk, which had been left on the inside of the stockings; which I did with a view of increasing the cohesion: whereas, when the inner sides of the stockings were put together, those ends of threads, and tufts of silk, by joining intimately with those of the different colour, probably contributed much to produce that powerful cohesion.

3°. When the inner stocking was drawn out, and applied to the outside of the other, they lifted one pound and three quarters; that is, between ten and eleven times the weight of the white stocking *.

It is not my design to draw any conclusions from these experiments, and much less to determine how far electrical cohesion may serve, as a principle, to account for many remarkable appearances in nature. I relate the experiments I have been witness to, by way of supplement to Mr. Symmer's third paper; and I consider the result of them, only, as a farther proof of the surprising degree, to which a power in electricity, which had not before been attended to, may be carried, in even the slightest substances, those of white and of black silk. I am, Sir,

Your most obedient, humble servant,

Kew, December 18, 1759.

John Mitchell.

* In the third of these papers I observed, that stockings electrified, and applied to one another externally, cohered with a force greater or less, according to the manner in which they joined in contact with each other. This appears to be the reason, why the stockings here made use of, being much less pliable than the thinner kind, do not, in external cohesion, raise a weight so great in proportion as those do. From thence I fancied, that if the stockings of spun silk should be first allowed to come together, and afterwards be pressed close between one's hands, their cohesion externally with one another would thereby be much improved: Accordingly, upon repeated trials, I found, that the white stocking, when thus pressed to the black in external contact, was capable of raising between three and four pounds; that is, about twenty-two times its own weight.

I have taken the liberty to subjoin these few observations, by way of notes, to Dr. Mitchell's letter, as they relate to the same subject, and contain matter, which has occurred since his letter was read in the Society.

February 1st, 1760.

VOL LI.

E e e

R. Symmer.

XXXVII.

XXXVII. *Some Observations relating to the Lyncurium of the Ancients ; by William Watfon, M. D. F. R. S.*

Read Dec. 20, 1759. **T**O determine the substance, denominated *Lyncurium* by the ancients, has been the occasion of much controversy among the more modern naturalists; some of whom, as late Dr. *Woodward*, believed it to be a species of the *belemnites*; others, as the late * *M. Geoffroy*, considered it as *amber*. But it is evident from † *Theophrastus*'s description of the *Lyncurium*, which is the most complete that has been handed down to us, that neither the one nor the other of the before-mentioned substances could be what he intended. His words are, Καὶ τὸ λυγκύριον. καὶ γὰρ ἐκ τέττε γλύφεται τὰ σφραγίδια. καὶ ἔστι σερροτάτη, καθάπερ λιθός. ἔλκει γὰρ ὥσπερ τὸ ἡλεκτρον. οἱ δὲ φάσιν ὁ μόνον κάρφη καὶ ξύλον, ἀλλὰ καὶ χαλκὸν καὶ σίδηρον, εἰ ἢ λεπτός. ὥσπερ καὶ Διοκλῆς ἔλεγεν. Ἔτι δὲ διαφανὴ τε σφόδρα καὶ πυρρὰ. . . . γίνεσθαι δὲ καὶ κληρονομία τίς αὐτῆς πλειων. From hence we learn, that “the *Lyncurium* was a stone
“ used for engraving seals on: that it was very hard:
“ that it was endowed with an attracting power like
“ amber: and that it was said, and by *Diocles*
“ among the rest, to attract not only straws and
“ small pieces of wood, but also copper and iron, if
“ beaten very thin: that it was pellucid, and of a

* *Mater. Med.* Vol. I. p. 165. de succino.

Idem et *Lyncurium* quoque dicitur.

† *Theophrastus* περὶ τῶν λίθων.

“ deep-red colour ; and required no small labour to
 “ polish it.” The rest of *Theophrastus*’s description
 is taken up with the fabulous account of the genera-
 tion of this stone, “ that it is formed by the urine of
 “ the lynx, which the animal, as soon as it parts
 “ with it, hides, and scrapes the earth together over
 “ it; and that the stones vary according to the sex
 “ and disposition of the animal.”

Dioscorides *, in his history of the *Lyncurium*,
 gives us only the fabulous history of its generation,
 before mentioned by *Theophrastus*; and subjoins, that
 it is called by some ἡλεκτρον πτερυγοφόρον; that is,
 amber, which attracts feathers to it.

Pliny, in his history †, disbelieves both the fabu-
 lous account of the generation of the *Lyncurium*, as
 well as its attractive quality, related both by *Diocles*
 and *Theophrastus*, and considers the whole as a falsity;
 though he is candid enough to confess, that neither
 himself, nor any one else in that age, had seen a gem
 of that appellation.

Theophrastus, though more ancient, is, in most
 particulars, more to be depended upon than either
Dioscorides or *Pliny*. He ought to be considered
 much more of an original author, and one who wrote
 from his own knowledge, than the others, who, va-
 luable as they are, must be regarded, in most respects,
 as compilers. His account, then, of the appearance
 and properties of the *Lyncurium* must be considered,
 in order to examine, if any substance, known in our

* Lib. II. cap. c.

† Plin. Hist. lib. XXXVII. cap. iii. Ego falsum id totum arbi-
 tor, nec visum in ævo nostro gemmam ullam eâ appellatione.

time, answers his description. But, first, it is plain that Dr. *Woodward's* hypothesis of the *belemnites* being the *Lyncurium*, was ill founded; inasmuch as the *belemnites* is neither pellucid, nor fit for engraving seals upon, on account of the friability of its texture; neither can it, by any management, be made to attract straws, chips of woods, or other light bodies. Nor is *Geoffroy's* opinion less liable to exception; as amber, though it has the attractive power mentioned by *Theophrastus*, yet it has by no means the firm texture requisite to have seals engraved upon it; neither is it so very hard, as is expressly said by this author concerning the *Lyncurium*, as to require great labour in polishing it. Add to these, that *Theophrastus* has given a *particular account* of the history and properties of *amber* * separately, in the before-mentioned work.

If, after what has been said, I may be permitted to give my thoughts concerning the *Lyncurium* of the ancients, I make no scruple to think it to be exceedingly probable, that what we now call the *Tourmaline* was the *Lyncurium* of *Theophrastus*, as it agrees with that author's description in all its sensible qualities; to wit, that it is a very hard pellucid stone, of a deep-red colour; that it is very proper to engrave seals upon; that it attracts, like amber, not only straws and light pieces of wood, but filings of iron and brass, as has been lately evinced by many experiments. And what will give some weight to this

* Vide *Theophrast.* περὶ τῶν λίθων. γγ.
 Καὶ τὸ ἡλεκτρον λίθος, καὶ γὰρ ὀρυκτὸν τὸ περὶ Λιγυστικὴν. καὶ τε ὡς ἂν ἡ
 τε ἔλκεν δύναμις ἀκολουθεῖν.

opinion is, that this stone, though not much attended to by us till very lately, is very common in several parts of the East Indies, and more particularly in the island of Ceylon, where it is called by the natives *Tournamal*.

The first account which we have had, of late years at least, of this extraordinary stone, was in the History of the Royal Academy of Sciences of Paris, for the year 1717; where we are told, that Mr. *Lemery* exhibited a stone, which, he said, was not common, and came from Ceylon. This stone attracted and repelled little light bodies, such as ashes, filings of iron, bits of paper, and such like. The publisher of that history then proceeds to give some reasons for these phænomena. *Linnaeus*, in his preface to the *Flora Zeylanica*, mentions this stone under the name of *lapis electricus*; and takes notice of M. Lemery's experiments before-mentioned.

Notwithstanding this, no further mention was made of this stone, and its effects, till very lately. The duke *de Noya*, in his letter to M. de Buffon, which was presented to the Royal Society a few months ago, informs us, that when at Naples in the year 1743, the late count *Pichetti*, secretary to the king, assured him, that, during his stay at Constantinople, he had seen a small stone, called a tourmaline, which attracted and repelled ashes. This account the duke *de Noya* had quite forgot; but, being last year in Holland, he saw and purchased two of these stones, which are there called *aschentrikker*. The making experiments with these called to his remembrance what formerly had been told him by count *Pichetti*. With these stones he made, in company with Mes-

sieurs

seurs *Daubenton* and *Adanson*, a great number of experiments, of which the duke has favoured the public with a particular account.

In the year 1757, there were two accounts published upon this subject: the one is a memoir of M. *Æpinus*, read to the Royal Academy at Berlin, intituled, *De quibusdam experimentis electricis notabilioribus*. The other is a treatise in quarto, printed at Rosstock, intituled, *Disputatio de electricitatibus contrariis. Auctore Joanne Carolo Wilke*. Since which time, Dr. Heberden, who is ever desirous of extending the bounds of science, having procured some of these stones from Holland, a great number and variety of experiments with them have been made here, particularly by the ingenious Mr. Wilton; an account of which he has very lately communicated to the Royal Society.

XXXVIII. *An Attempt to account for the regular diurnal Variation of the horizontal magnetic Needle; and also for its irregular Variation at the Time of an Aurora Borealis: By John Canton, M. A. and F. R. S.*

Read Dec. 13, 1759. **T**HE late celebrated Mr. George Graham made a great number of observations on the diurnal variation of the magnetic needle, in the years 1722 and 1723; but declared himself ignorant of the cause of that variation, in No 383 of the Philosophical Transactions, where many of those observations are to be found. About
2 the

the year 1750, Mr. Wargentin, secretary of the Royal Academy of Sciences in Sweden, took notice, both of the regular diurnal variation of the needle, and also of its being disturbed at the time of an aurora borealis, as recorded in the 47th volume of the Philosophical Transactions; but is silent as to the cause. I had no opportunity of making observations of this sort myself, till the latter end of the year 1756; but, since that time, I have made near four thousand, with an excellent variation-compass, of about nine inches in diameter. The number of days, on which these observations were taken, is 603; and the diurnal variation on 574 of them was *regular*; that is, the absolute variation of the needle westward, was *increasing* from about eight or nine o'clock in the morning, till about one or two in the afternoon, when the needle became stationary for some time; after that, the absolute variation westward was *decreasing*, and the needle came back again to its former situation, or near it, in the night, or by the next morning. The diurnal variation is *irregular*, when the needle moves slowly eastward in the latter part of the morning, or westward in the latter part of the afternoon; also when it moves much either way after night, or suddenly both ways within a short time. These irregularities seldom happen more than once or twice in a month, and are always accompanied (so far as I have been able to observe) with an aurora borealis. Thus having explained what I mean by the regular and irregular diurnal variation, and shewn, that this variation is generally regular; I shall now, in the first place, endeavour to account experimentally for the regular variation; then offer a conjecture concerning
the

the cause of the irregular variation; and, lastly, attempt to make it appear probable, that the aurora borealis arises from the same cause.

The attractive power of the magnet, (whether natural, or artificial) will *decrease* while the magnet is *heating*, and *increase* while it is *cooling*; as will appear by the following experiments.

Experiment 1. About E. N. E. from a compass, a little more than three inches in diameter, I placed a small magnet two inches long, half an inch broad, and three-twentieths of an inch thick, parallel to the magnetic meridian; and at such a distance, that the power of the south end of the magnet was but just sufficient to keep the north end of the needle to the N. E. point, or to 45° . The magnet being covered by a brass weight of sixteen ounces, about two ounces of boiling water was poured into it, by which means the magnet was gradually heating for seven or eight minutes; and during that time, the needle moved about three quarters of a degree westward, and became stationary at $44^\circ \frac{1}{4}$; in nine minutes more, it came back a quarter of a degree, or to $44^\circ \frac{1}{2}$; but was some hours before it gained its former situation, and stood at 45° . *N. B.* The greater the power of the same magnet, the more it will lose in a given degree of heat.

Exp. 2. On each side of the compass, and parallel to the magnetic meridian, I placed a strong magnet of the size above-mentioned; so that the south ends of both the magnets acted equally on the north end of the needle, and kept it in the magnetic meridian; but if either of the magnets was removed, the needle was attracted by the other, so as to stand at 45° degrees.

grees. The magnets were both covered with brass weights of sixteen ounces each. Into the eastern weight I poured about two ounces of boiling water; and the needle in one minute moved half a degree, and continued moving westward for about seven minutes, when it arrived at $2^{\circ} \frac{3}{4}$. It was then stationary for some time; but, in twenty-four minutes from the beginning, it came back to $2^{\circ} \frac{1}{2}$, and in fifty minutes to $2^{\circ} \frac{1}{4}$. I then filled the western weight with boiling water, and in one minute, the needle came back to $1^{\circ} \frac{1}{4}$; in six minutes more, it stood half a degree eastward; and after that, in about forty minutes, it returned to the magnetic north, or its first situation.

It is evident, that the magnetic parts of the earth in the north on the east side, and the magnetic parts of the earth in the north on the west side of the magnetic meridian, equally attract the north end of the needle. If then the eastern magnetic parts are heated faster by the sun in the morning, than the western, the needle will move westward, and the absolute variation will increase; when the attracting parts of the earth on each side the magnetic meridian have their heat increasing equally, the needle will be stationary, and the absolute variation will then be greatest; but, when the western magnetic parts are either heating faster, or cooling slower than the eastern, the needle will move eastward, or the absolute variation will decrease; and when the eastern and western magnetic parts are cooling equally fast, the needle will again be stationary, and the absolute variation will then be least. This may be still further illustrated, by placing the compass and two magnets, as in the last experi-

ment, behind a screen near the middle of the day in summer ; then, if the screen be so moved, that the sun may shine only on the eastern magnet, the needle will sensibly vary in its direction, and move towards the west ; and if the eastern magnet be shaded, while the sun shines on the western, the needle will move the contrary way. By this theory, the diurnal variation in the summer ought to exceed that in the winter ; and I accordingly find by observation, that the diurnal variation in the months of June and July, is almost double that of December and January.

The irregular diurnal variation must arise from some other cause than that of heat communicated by the sun ; and here I must have recourse to subterranean heat, which is generated without any regularity as to time, and which will, when it happens in the north, affect the attractive power of the magnetic parts of the earth on the north end of the needle. The reverend Dr. Hales has a good observation on this heat, in the Appendix to the second volume of his Statical Essays, which I shall here transcribe. “ That the warmth of
 “ the earth, at some depth under-ground, has an
 “ influence in promoting a thaw, as well as the
 “ change of the weather from a freezing to a thaw-
 “ ing state, is manifest from this observation ; viz.
 “ Nov. 29, 1731, a little snow having fallen in the
 “ night, it was, by eleven the next morning, mostly
 “ melted away on the surface of the earth, except in
 “ several places in *Bushy-Park*, where there were
 “ drains dug, and covered with earth, where the
 “ snow continued to lie, whether those drains were
 “ full of water, or dry ; as also where elm-pipes lay
 “ under-ground ; a plain proof that these drains inter-
 “ cepted

“ cepted the warmth of the earth from ascending
 “ from greater depths below them; for the snow lay
 “ where the drain had more than four feet depth of
 “ earth over it. It continued also to lie on thatch,
 “ tiles, and the tops of walls.”

That the air nearest the earth will be most warmed by the heat of it, is obvious; and this has frequently been taken notice of in the morning, before day, by means of thermometers at different distances from the ground, by the reverend Dr. Miles, at Tooting in Surrey; and is mentioned in p. 526, of the 48th volume of the Philosophical Transactions.

The aurora borealis, which happens at the time the needle is disturbed by the heat of the earth, is supposed to be the electricity of the heated air above it; and this will appear chiefly in the northern regions, as the alteration in the heat of the air in those parts will be greatest. This hypothesis will not seem improbable, if it be considered, that electricity is now known to be the cause of thunder and lightning; that it has been extracted from the air at the time of an
 “ aurora borealis; that the inhabitants of the northern countries observe the aurora to be remarkably strong, when a sudden thaw happens after severe cold weather; and that the curious in these matters, are now acquainted with a substance, that will, without friction, both emit and absorb the electrical fluid, only by the increase, or diminution of its heat: for if the *Tourmalin* be placed on a plane piece of heated glass, or metal, so that each side of it, by being perpendicular to the surface of the heating body, may be equally heated; it will, while heating, have the electricity of one of its sides positive, and that of the other

negative; this will likewise be the case when it is taken out of boiling water, and suffered to cool; but the side that was positive while it was heating, will be negative while it is cooling, and the side that was negative, will be positive*.

For the sake of those who may be desirous of examining the diurnal variations of the needle very minutely, I shall annex a complete year's observations; and shall deduce, from the regular variations during that time, the mean diurnal variation belonging to each month: whence it will appear, that the diurnal variation increases from January to June, and decreases from June to December.

In the following table, the first column contains the day of the month; and the second, the hour and minute of the day (according to equal time), when each observation was made. The third column contains the absolute variation of the needle westward, or the angle it made with a true meridian line at the respective times; which differs less than two minutes of a degree from that at Greenwich, where the reverend Dr. Bradley was so kind as to give me several opportunities of taking it, in his presence, by the curious apparatus for that purpose, belonging to the royal observatory. And in the fourth column are set down the degrees of heat, by a Fahrenheit's thermometer, hung without, in the shaded air, on the north side of the house.

* See the Gentleman's Magazine for last September.

Spital Square, London, 1759.

January.					January.						
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
1	I	M	0	18	54 34	6	0	A	5	19	6 39
	10		15	18	56 37		I	10	19	6 40	
	II		0	18	57 38		I	30	19	5 40	
	0	A	15	18	59 40		2	30	19	4 39	
	I		0	19	0 42		9	10	18	54 33½	
	I		45	18	59 42	7	II	M	0	18	59 42
	8		0	18	54 40		0	A	30	19	0 44
2	10	M	30	18	56 45		I	0	19	I	45
	I	A	12	18	59 46		I	30	19	0 47	
3	0	M	10	18	54 45		2	30	18	59 46	
	10		0	18	57 45		8	50	18	55 44	
	0	A	20	19	2 44½	8	0	M	40	18	53 45
	I		0	19	3 44		8	45	18	53 47	
	I		30	19	4 44		9	45	18	55 47½	
	2		0	19	3 44		I	A	25	19	I 50
	4		0	18	59 45		4	50	18	56 48	
	9		0	18	54 46		9	10	18	52 47	
4	0	M	18	18	54 47	9	0	M	25	18	54 46
	9		30	18	56 49		8	53	18	57 44	
	II		58	18	58 52		0	A	30	19	0 46
	I	A	55	18	58 53		I	15	19	0 47	
	3		12	18	58 52½		9	0	18	57 49	
5	0	M	30	18	54 51		II	30	19	0 52	
	10		0	18	57 47		II	40	19	2 52	
	0	A	30	19	0 46	10	9	M	0	18	55 52
	I		15	19	I 45½		10	10	18	56 52½	
	2		20	19	0 45		I	A	0	18	59 54
	4		0	18	57 44		I	30	18	59 54½	
	9		30	18	54 42		2	20	18	59 54	
6	0	M	20	18	54 40		3	0	18	58 54	
	10		15	19	2 37		9	10	18	55 53½	
	10		45	19	4 38		II	45	18	55 52	

Tanu.

January.						January.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
11	8	M 40	18	54	48	15	3	A 50	18	58	51
	9	40	18	55	49		9	10	18	55	50
	10	40	18	58	49	16	0	M 25	18	55	50
	11	50	19	0	49½		8	20	18	55	48
	0	A 40	19	2	51		9	20	18	55	49
	1	20	19	2	51		10	30	18	57	49½
	2	10	19	1	51		11	30	18	59	50
	2	45	19	0	51		1	A 0	19	2	50
12	0	M 15	18	57	47		2	55	19	0	49
	9	0	18	54	48		5	50	18	59	48½
	10	10	18	56	48		9	0	18	58	48
	11	5	18	58	48½	17	0	M 10	18	54	47
	0	A 5	19	0	49½		9	0	18	55	44
	1	20	19	1	50		10	15	18	56	45
	2	30	19	0	50		0	A 20	18	59	46
	5	15	18	57	49½		1	10	19	0	46½
	8	30	18	56	48		2	45	18	59	46½
13	0	M 30	18	56	46		6	0	19	1	46½
	9	20	18	55	47		9	0	18	48	46½
	10	20	18	56	47		9	5	18	45	46½
	11	50	18	58	48		9	20	18	46	46½
	1	A 15	19	0	45		9	30	18	47	46½
	2	30	18	59	44	18	0	M 20	18	50	46
	7	0	18	57	42		8	30	18	54	48½
14	0	M 10	18	58	43		10	0	18	55	48½
	9	45	18	56	46		11	30	18	58	49
	10	45	18	57	50		0	A 10	19	0	49½
	9	A 10	18	55	48		1	55	19	1	50
15	0	M 10	18	55	47		5	20	18	59	47
	8	30	18	55	48	19	0	M 5	18	55	43
	10	20	18	56	51		8	45	18	56	37
	11	35	18	58	52		11	0	18	59	39
	0	A 45	19	0	52½		0	A 20	19	1	39
	1	35	19	0	52		0	45	19	3	39½

January.						January.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
19	1	A	20	19	4 39	22	2	A	45	19	2 36½
	2		15	19	3 39		3		45	19	0 35
	3		35	19	1 38		8		0	18	58 32
	6		15	19	4 37		9		10	18	57 34
	6		32	19	0 37	23	0	M	10	18	59 31
	6		53	19	0 36		8		50	18	55 28
	9		0	18	59 34		9		50	18	56 29
	0	M	5	18	57 32½		11		25	18	59 33
20	8		35	18	56 32½	1	A	10	19	2 37	
	9		45	18	58 34	3		45	19	3 36½	
	0	A	45	19	3 38	5		10	19	0 35	
	1		25	19	4 40	7		0	18	59 34	
	2		10	19	2 39½	9		0	18	56 33	
	7		15	18	58 37	24	0	M	25	18	56 32
	8		45	18	54 36		9		0	18	57 35
	9		20	18	54 35		10		0	18	59 38
0	M	10	18	58 33	11			45	19	1 41	
21	10		0	18	57 32	0	A	15	19	2 42	
	11		0	18	59 33	0		40	19	3 42	
	12		0	19	1 35	1		15	19	4 42	
	1	A	0	19	2 37	2		0	19	4 42½	
	1		35	19	2 37½	3		0	19	4 43	
	2		45	19	1 36½	3		55	19	2 41	
	5		5	19	0 34	6		25	18	59 38	
	9		55	18	57 31	25	0	M	25	18	50 38
0	M	30	18	55 28½	9			0	18	54 38	
8		30	18	54 28	11			0	18	58 43	
9		30	18	55 29	0		A	20	19	0 44	
22	10		40	18	58 32	1		20	19	1 45	
	11		40	19	0 35	1		45	19	1 45	
	0	A	30	19	2 36½	2		20	19	1 45	
	1		10	19	4 37	4		0	18	59 44½	
	1		35	19	4 37	7		45	18	57 44	
	2		10	19	3 37	26	0	M	20	18	56 42

January.						January.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
26	8	M	35	18 55	44	30	0	M	25	18 57	41
	10		40	18 56	46		9		0	18 56	44
	0	A	30	19 1	47		10		0	18 54	46
	1		25	19 4	47½		11	10	18 57	47	
	2		20	19 3	47		1	A	0	19 3	48½
	4		30	19 0	46		6		30	18 58	47½
	9		0	18 57	45		11		55	18 54	47
	11		45	18 56	44	31	8	M	30	19 0	45
27	8	M	40	18 53	43		10		0	18 57	46
	10		0	18 54	44		11		0	18 58	47
	11		30	18 58	46		12		0	18 59	48
	1	A	7	19 0	48		0	A	58	19 1	49
	1		30	19 0	49		1		50	19 3	49
	2		10	19 1	49		2		15	19 2	49
	3		35	18 59	48		4		0	18 59	48½
	8		20	18 57	47		7		10	18 58	47
	11		55	18 56	47		11		55	18 57	46
28	10	M	0	18 55	49	February.					
	11		0	18 56	49	D.	H.	M.	°	'	Th.
	1	A	0	19 1	49	1	8	M	40	18 54	39
	1		30	19 2	49½		9		20	18 54	40
	2		40	19 1	47		10		0	18 55	41
	6		45	18 59	43		11		45	18 58	43
	8		50	18 56	42		0	A	50	19 0	45
29	0	M	25	18 56	39		1		30	19 0	46
	8		40	18 54	35		2		12	18 59	46
	10		15	18 55	38		4		40	18 57	44
	11		40	18 58	42		11		40	18 56	40
	1	A	0	19 2	45	2	9	M	15	18 55	46
	1		25	19 2	45		0	A	10	19 0	49
	2		0	19 3	45		1		15	19 1	50
	2		45	19 2	44		2		10	19 3	49½
	3		35	19 0	43½						
	9		0	18 58	42						

Febru-

February.						February.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
2	2	A 40	19	2	49		0	A 9	18	57	46
	4	30	18	59	48½		0	18	19	1	46½
	9	20	18	57	48		0	25	19	6	46½
3	0	M 23	18	56	48		0	31	19	8	47
	8	30	18	57	49		2	0	19	1	48
	9	30	18	57	49		3	0	18	36	47½
	10	15	18	58	49½		4	25	18	39	46½
	0	A 30	19	6	50		4	26½	18	54	46½
	1	5	19	7	50		4	28	19	4	46½
	1	40	19	8	49		4	29½	19	44	46½
	9	0	18	51	43		4	30½	19	46	46½
4	0	M 10	18	58	40		4	32½	19	49	46½
	9	30	18	54	42		4	34	19	4	46½
	11	0	18	58	45		4	35½	19	19	46½
	11	20	18	24	45½		4	36½	19	24	46½
	11	22	18	16	45½		4	38	19	34	46½
	11	22½	18	12	45½		4	38½	20	4	46½
	11	24	18	19	45½		4	39½	20	54	46½
	11	25½	18	14	45½		4	41	21	24	46½
	11	27	18	21	45½		4	42¾	20	44	46½
	11	29	18	34	45½		4	44½	19	24	46½
	11	30	18	29	45½		4	45½	17	54	46½
	11	31½	18	29	45½		4	47	18	14	46½
	11	32½	18	24	45½		4	48½	18	32	46½
	11	33	18	19	45½		4	51¼	17	29	46½
	11	35	18	24	45½		4	52¾	17	54	46½
	11	36	18	24	45½		4	54	18	24	46½
	11	38½	18	21	45½		5	7½	17	54	46
	11	42	18	16	45½		5	9½	17	50	46
	11	46	18	34	46		5	39½	18	37	46
	11	48½	18	36	46		5	51	19	9	45½
	11	53	18	45	46		6	44	19	2	44
	12	0	18	51	46		6	53	18	57	43½
	0	A 6	18	54	46		8	40	19	2	42

A strong Aurora Borealis.

February.						February.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
An Aurora Bor.	4	10 A	10	18 59	41	8	9 A	25	19	1	46
	5	8 M	32	19 11	43	9	0 M	10	18 59		46
		11	8	18 59	45		8	55	18 58		45
		11	58	19 2	47		10	0	18 58		45½
		0 A	30	18 58	49		11	40	19 0		46
		1	40	19 4	50		0 A	55	19 4		46½
		6	0	18 55	48		1	50	19 5		47
		6	42	18 49	48		2	15	19 4		47
		7	24	18 44	48		5	35	19 0		45
		7	28	18 46	48		9	15	18 58		43
		8	50	18 54	48	10	0 M	50	18 57		42
		0 M	14	18 55	48½		8	35	18 56		41
		8	30	18 55	48		10	0	18 56		43
		10	0	18 55	48½		11	30	18 59		44
		11	55	19 1	49		1 A	15	19 4		45
		1 A	5	19 5	50		2	10	19 6		44
		2	45	19 4	49		8	30	19 2		41
		5	20	18 57	47	11	1 M	10	18 58		42
		8	0	18 56	44		10	0	18 58		47
		11	50	18 50	42		11	0	18 59		49
	7	8 M	40	18 55	41		0 A	5	19 1		51
		9	50	18 56	43		1	30	19 3		51½
		11	20	19 0	45		2	30	19 4		51
		1 A	17	19 4	49		4	20	19 0		47
		2	15	19 6	49		7	30	18 59		44
		6	30	19 1	48		10	5	18 58		43
		11	55	18 57	47	12	0 M	45	18 59		42
8		8 M	40	18 56	46		8	40	18 55		41
		10	10	18 56	48		10	0	18 55		43
		11	10	18 57	48½		11	0	18 56		45
		0 A	30	19 2	49		12	0	18 59		48
		1	40	19 3	49		1 A	0	19 2		49
		2	20	19 3	48½		2	0	19 4		49
		3	55	19 2	48		2	45	19 3		48

February.							February.						
D.	H.	M.	°	'	Th.		D.	H.	M.	°	'	Th.	
12	3	A 45	19	3	47		17	0	M 5	18	56	40	
	6	20	18	58	43			8	45	18	54	37	
	9	0	18	57	42			9	50	18	55	38	
	11	50	18	55	40		11	0	18	57	40		
13	1	M 0	18	55	38			0	A 5	19	0	44	
	8	50	19	5	37			1	15	19	2	46	
	9	57	19	2	39			2	10	19	2	43	
	10	50	19	4	43			3	12	19	1	46	
	0	A 35	19	3	48			7	40	18	59	40	
	1	10	19	3	49		18	0	M 30	18	57	35	
	3	20	19	4	48			9	55	18	57	40	
	5	30	18	59	43		11	5	18	59	43		
	6	30	18	59	42			0	A 10	19	1	46	
	9	10	18	57	41			1	5	19	2	48	
	11	38	18	57	41			2	15	19	2	47	
14	8	M 50	18	55	40			4	30	19	0	45	
	10	10	18	55	40			9	45	18	58	41	
	11	50	18	58	40 $\frac{1}{2}$		19	0	M 30	18	58	42	
	2	A 40	19	2	42			8	35	18	56	40	
	7	25	18	58	38		10	0	18	56	40 $\frac{1}{2}$		
15	0	M 30	18	57	34		11	0	18	58	41		
	8	55	18	55	34 $\frac{1}{2}$			0	A 15	19	1	42	
	9	55	18	55	37			1	35	19	4	44	
	11	0	18	57	39			2	10	19	5	43	
	0	A 20	19	1	41			3	0	19	5	42	
	1	40	19	5	43			4	0	19	4	41	
	3	15	19	4	42			7	10	19	1	40	
16	0	M 5	18	57	40			9	4	18	47	38 $\frac{1}{2}$	
	9	40	18	55	40			9	5	18	43	38 $\frac{1}{2}$	
	11	45	18	58	42			9	7	18	43	38 $\frac{1}{2}$	
	1	A 0	19	0	43			9	25	18	31	38	
	2	15	19	4	43			9	27	18	29	38	
	5	45	19	0	42		11	40	18	47	38		
	9	5	18	56	41		20	8	M 50	18	54	39	

February.						February.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
20	9	M 55	18	54	40	24	11	M 15	18	56	45
	11	0	18	56	41		1	A 40	19	3	40
	0	A 25	19	1	41½		2	40	19	4	40
	1	15	19	4	42		3	20	19	2	40
	2	30	19	4	41½		4	0	19	1	39
	6	20	19	5	41		7	5	18	59	39
	9	25	18	53	40		9	15	18	49	40
21	1	M 20	18	56	40		11	58	18	57	40
	8	50	18	54	39	25	10	M 0	18	55	40½
	10	0	18	55	39		0	A 28	18	59	41
	12	0	18	58	39		1	55	19	3	41
	1	A 40	19	2	42		3	20	19	2	41
	2	30	19	1	41½		4	20	19	0	42
	4	30	18	59	38		6	25	18	59	42
	8	0	18	57	36		9	55	18	59	43
22	0	M 55	18	58	39	26	9	M 0	18	53	46
	9	5	18	55	40		10	0	18	53	48
	11	0	18	56	44		11	5	18	55	50
	1	A 0	19	0	48		0	A 18	19	0	52
	2	5	19	3	47½		1	30	19	3	54
	3	10	19	2	47		2	22	19	3	53
	5	15	18	59	46½		4	5	19	2	50
	9	25	18	58	46		8	0	18	58	49
23	0	M 25	18	58	45		11	53	18	58	48
	8	50	18	55	43	27	9	M 23	18	59	49
	9	55	18	55	44		10	10	19	1	50
	11	5	18	57	44		11	25	19	2	51
	0	A 13	19	1	43		1	A 25	19	9	52
	1	42	19	4	43		2	15	19	10	52½
	3	8	19	3	43		2	58	19	4	53
	5	22	18	59	43		3	56	19	3	52
	9	40	18	58	40		5	50	19	3	51
24	0	M 50	18	59	38		8	30	18	54	50
	10	0	18	55	44		9	8	18	52	49

February.						March.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
28	0	M 18	18	54	46	3	11	M 5	18	57	45
	10	25	18	53	43		0	A 20	19	3	46 $\frac{1}{2}$
	11	25	18	57	45		1	7	19	7	48
	0	A 25	19	0	47		2	0	19	4	49
	1	10	19	3	49		3	5	19	2	48
	1	57	19	4	48		8	20	18	58	43 $\frac{1}{2}$
	7	30	18	59	46		11	55	18	54	41 $\frac{1}{2}$
	9	5	18	59	45	4	9	M 30	18	54	42 $\frac{1}{2}$
	11	45	18	58	44		11	0	18	56	44
March.							0	A 10	19	1	46
							1	43	19	4	45 $\frac{1}{2}$
							2	40	19	4	46
							4	30	19	2	45 $\frac{1}{2}$
							6	50	18	59	43 $\frac{1}{2}$
							10	10	18	57	42 $\frac{1}{2}$
						5	0	M 55	18	57	43
							9	0	18	55	42 $\frac{1}{2}$
							11	10	18	59	45 $\frac{1}{2}$
							0	A 28	19	4	46 $\frac{1}{2}$
							1	50	19	5	47
							2	40	19	6	46
							3	56	19	4	44
							7	15	19	0	42 $\frac{1}{2}$
						6	0	M 45	18	58	42
							8	40	18	56	42
							9	30	18	56	43
							11	23	19	0	43 $\frac{1}{2}$
							0	A 10	19	2	43 $\frac{1}{2}$
							1	25	19	4	43
							2	0	19	5	42 $\frac{1}{2}$
							4	0	19	3	42
							6	20	19	0	42 $\frac{1}{2}$
							9	40	18	58	43
							11	53	18	57	40 $\frac{1}{2}$

March.

March.						March.							
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.		
7	9	M	15	19	16	42	10	2	A	45	19	2	44
	9		50	19	12	43		3		35	19	1	43 $\frac{1}{2}$
	11		25	19	3	45		4		32	19	0	44 $\frac{1}{2}$
	0	A	19	19	12	46		7		0	18	59	41
	0		43	19	8	47	11	11		47	18	59	36
	1		10	19	4	46 $\frac{1}{2}$		10	M	10	18	58	42 $\frac{1}{2}$
	2		15	19	3	47 $\frac{1}{2}$		11		7	19	0	43
	3		0	19	5	45 $\frac{1}{2}$		12		0	19	1	43
	5		10	19	2	42 $\frac{1}{2}$		1	A	15	19	2	44 $\frac{1}{2}$
	7		50	18	59	40		2		35	19	1	44
8	0	M	10	18	57	39	12	7		32	18	59	40
	8		38	18	58	40		0	M	50	18	59	36
	10		0	18	57	43		8		47	18	57	37 $\frac{1}{2}$
	11		20	18	59	45		10		30	18	59	43
	0	A	15	19	0	46	13	0	A	28	19	4	47
	1		26	19	1	47		1		55	19	5	49 $\frac{1}{2}$
	2		20	19	0	46 $\frac{1}{2}$		3		15	19	3	47 $\frac{1}{2}$
	4		40	18	58	43		5		5	19	1	46 $\frac{1}{2}$
	6		30	18	58	42		0	M	22	18	59	40
	0	M	5	18	54	43		8		56	18	57	42
9	8		30	18	57	40	10	10		0	18	59	45
	9		55	18	55	41		11		20	19	3	48 $\frac{1}{2}$
	11		55	19	2	45		0	A	25	19	6	49 $\frac{1}{2}$
	1	A	12	19	4	48		1		5	19	7	51
	2		5	19	4	49	14	1		28	19	7	52
	4		7	18	59	48 $\frac{1}{2}$		6		45	18	59	46
	6		35	18	58	46		1	M	17	18	59	42
	8		50	18	59	46		8		45	18	56	48
	11		30	19	0	44 $\frac{1}{2}$		10		0	18	57	51
	9	M	20	18	58	44		11		20	18	59	52
10	10		15	18	59	44	0	A	52	19	4	53	
	11		30	19	2	42	2		20	19	5	53	
	0	A	52	19	3	42	3		5	19	3	53	
	1		30	19	4	42 $\frac{1}{2}$	5		35	19	0	51	

March.

March.						March.							
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.		
14	9	A	5	18	59	50	19	8	M	40	18	54	37
15	0	M	7	18	59	49½	10	15	18	56	42		
	8		30	18	55	48	11	20	19	0	43½		
	10		10	18	54	49	0	A	45	19	7	43½	
	11		5	18	56	50	1	22	19	14	42½		
	0	A	20	19	0	51	1	58	19	13	42		
	1		42	19	1	51	2	18	19	11	41½		
	2		35	19	1	51	3	10	19	9	41		
	3		30	19	0	50½	4	0	19	8	41		
	11		55	18	57	50	5	15	18	58	41½		
16	8	M	30	18	53	50½	5	45	18	54	41½		
	10		0	18	54	51	6	33	18	58	43		
	11		10	18	58	52	7	17	19	0	44		
	0	A	16	19	2	53	11	55	18	46	46		
	1		15	19	4	49	20	7	M	55	18	54	45½
	2		20	19	5	47	8	46	18	54	47		
	3		55	19	1	47½	9	46	18	56	48½		
	6		20	18	59	45	10	46	18	58	49		
	9		5	18	58	43	11	46	19	2	49		
17	0	M	35	18	59	40	1	A	0	19	11	48½	
	8		5	18	54	39	1	46	19	9	48		
	9		10	18	53	43	2	58	19	8	49		
	10		15	18	55	45½	3	59	19	6	47½		
	11		20	19	0	48	5	0	19	2	47		
	0	A	25	19	4	49	6	0	19	0	45		
	1		35	19	7	44	8	30	18	55	44½		
	4		0	19	2	46	21	1	M	12	19	4	39½
	8		45	18	58	40½	8	55	18	56	40½		
18	0	M	10	18	57	39½	10	0	18	55	42½		
	9		50	18	55	43	11	0	18	59	42½		
	11		0	18	59	44	0	A	10	19	3	43½	
	4	A	56	19	7	40	1	5	19	5	44½		
	6		37	18	54	39½	1	50	19	6	46		
19	0	M	10	18	55	37	2	50	19	4	45½		

March.

March.						March.								
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.			
21	4	A	0	19	1	45	24	9	A	5	19	0	45	
	5		30	18	58	43	25	0	M	35	19	1	41	
	8		5	18	57	40		10		0	18	58	45½	
	11		50	18	57	37½		11		0	19	0	46½	
	9	M	0	18	54	44		0	A	5	19	2	48	
	10		0	18	56	46		1		45	19	4	52	
	11		0	19	0	48½		2		30	19	3	51½	
	12		0	19	3	52½		4		35	19	1	51	
	1	A	0	19	6	52		6		43	19	0	49	
	1		30	19	6	52½		9		38	18	59	48	
	2		0	19	6	53	26	0	M	15	18	59	47½	
	3		0	19	6	54		8		30	18	55	47	
22	4		30	19	8	52		9		20	18	56	48	
	7		53	18	48	48		10		5	18	58	49	
	9		0	18	26	48½		11		10	19	3	50	
	0	M	50	18	53	44		0	A	5	19	6	51	
	9		0	18	59	48		1		12	19	7	51½	
	10		0	19	0	50		1		45	19	6	52	
	11		0	19	2	53		3		0	19	2	52½	
	12		0	19	4	54½		3		50	19	0	52½	
	1	A	0	19	5	55		6		56	18	59	49	
	2		0	19	5	56		9		2	18	59	46½	
	3		0	19	4	57	27	0	M	5	18	59	44	
	4		0	19	2	56		8		12	18	53	48	
23	5		0	19	1	54		9		5	18	52	50	
	9		0	18	59	50		10		4	18	56	51½	
	0	M	38	18	58	47		11		15	19	2	53½	
	8		54	18	54	50		0	A	10	19	5	55	
	10		5	18	56	50		1		15	19	7	57	
	11		3	19	0	50½		2		50	19	6	56½	
	0	A	2	19	4	52		4		0	19	3	57	
	1		5	19	5	51		5		0	18	59	56	
	2		2	19	7	51½		6		25	18	59	54	
	3		1	19	4	52½		9		40	18	58	50	
	An Aurora Bor.													
24														
25														
26														
27														
28														
29														
30														
31														
An Aurora Bor.														

March.						March.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
28	0	M	40	18 58	49	31	0	M	10	18 59	35½
	8		15	18 52	49		8		25	18 54	35
	9		15	18 54	49½		9		15	18 53	36
	10		0	18 55	50½		10		10	18 54	37½
	0	A	2	19 3	51		11		5	18 58	40
	1		3	19 7	52		0	A	10	19 5	42
	1		47	19 8	52½		1		5	19 8	43
	3		12	19 6	52½		1		35	19 9	43½
	4		20	19 3	52		2		22	19 7	43½
	5		5	19 1	52		4		10	19 3	43½
	8		0	18 59	50		8		10	19 0	40
	9		10	18 54	49	April.					
29	0	M	15	18 58	47	D.	H.	M.	°	'	Th.
	8		10	18 50	49	1	0	M	40	18 59	37
	9		10	18 51	48½		9		50	18 55	50½
	10		10	18 54	51		11		0	18 58	55
	11		20	19 1	52		0	A	2	19 2	55
	0	A	32	19 7	52		1		30	19 6	54½
	1		45	19 9	52		2		0	19 6	55
	2		15	19 9	52		3		2	19 4	54
	3		4	19 6	51		5		15	19 0	54
	4		5	19 2	48½		6		5	18 59	52½
30	0	M	10	18 59	40		9		45	18 59	48
	8		7	18 52	41	2	0	M	40	18 58	45
	9		5	18 52	41		7		50	18 53	44
	10		20	18 56	43		9		10	18 54	48½
	11		15	19 0	45		10		0	18 56	51
	0	A	20	19 6	45½		0	A	10	19 3	55
	1		15	19 8	46		1		3	19 5	57
	1		57	19 8	46½		1		0	19 6	56½
	3		10	19 6	45		2		10	19 6	56
	4		12	19 3	43		3		0	19 5	55
	5		30	19 1	42						
	9		35	18 59	37						

April.					
D.	H.	M.	°	'	Th.
2	4	A	0	19	2 53 $\frac{2}{3}$
	4		56	19	1 53
	7		20	18	59 47
3	0	M	35	18	58 41
	8		10	18	54 43
	9		15	18	54 48 $\frac{1}{2}$
10	0		0	18	56 51
	11		4	19	0 53
	0	A	2	19	4 55
1	0		0	19	7 56
	1		20	19	7 56 $\frac{1}{2}$
	4		11	19	2 56
6	6		15	18	58 52
	8		43	18	58 46
	0	M	40	18	58 42
4	8		15	18	51 47
	9		10	18	51 48
	10		6	18	54 50
11	7		7	19	0 52 $\frac{1}{2}$
	12		0	19	5 50 $\frac{1}{2}$
	1	A	0	19	6 52
1	30			19	7 53
	2		8	19	7 54
	3		13	19	4 54 $\frac{1}{2}$
4	20			19	1 54
	5		20	19	0 53
	8		45	18	54 50
5	0	M	5	18	53 49 $\frac{1}{2}$
	8		10	19	24 51
	9		8	19	18 53
10	15			19	9 56
	11		10	19	10 57 $\frac{1}{2}$
	0	A	7	19	15 58
1	0			19	15 58

April.					
D.	H.	M.	°	'	Th.
5	1	A	30	19	16 58
	2		20	19	19 57
	4		12	19	12 56
7	7		30	19	1 52
	0	M	11	19	1 48
	8		10	18	54 50
9	9		9	18	55 50 $\frac{1}{2}$
	10		12	18	58 51
	11		25	19	3 51
0	A		10	19	4 49 $\frac{1}{2}$
	1		30	19	5 47
	2		15	19	4 46
3	18			19	2 45
	5		18	18	58 46 $\frac{1}{2}$
	1	M	27	18	58 42
7	8		25	18	53 43 $\frac{1}{2}$
	9		16	18	54 46
	10		20	18	57 49 $\frac{1}{2}$
11	5			18	59 51
	0	A	16	19	4 50 $\frac{1}{2}$
	1		0	19	5 51
2	1			19	5 51
	2		40	19	3 52
	9		1	18	59 44 $\frac{1}{2}$
8	0	M	25	18	59 42 $\frac{1}{2}$
	9		48	18	58 47
	11		3	19	0 50
0	A		10	19	3 51
	1		45	19	5 49
	2		35	19	4 49
4	10			19	2 46
	7		46	19	0 48
	0	M	37	18	59 48
9	9		9	18	54 51 $\frac{1}{2}$

April

April.						April.									
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.				
9	10	M	5	18	56	54	12	2	A	0	19	7	54		
	11		6	18	59	54½		3		1	19	5	54½		
		0	A	5	19	4	57		9		5	19	0	48	
		1		5	19	9	57	13	0	M	23	18	56	45	
		1		40	19	8	56		9		30	18	57	50	
		3		10	19	6	55		10		8	13	59	51½	
		4		7	19	3	54½		11		7	19	3	53½	
		6		45	19	0	53½			0	A	12	19	7	55
		11		40	18	59	46½		1		30	19	12	56	
	10	9	M	10	18	53	47		1		58	19	14	57	
10			1	18	55	48½		2		10	19	15	57		
11			3	18	59	49½		2		24	19	13	57		
		0	A	2	19	1	51		3		10	19	8	57	
		1		10	19	6	53		4		20	19	6	56	
		1		43	19	7	53		5		35	19	4	54½	
		5		55	19	1	50		6		20	19	2	53	
		7		50	18	59	46		9		22	18	44	49	
10			0	18	57	44		14	0	M	57	18	53	44	
		0	M	55	18	57	42		9		0	18	52	48	
11	9		20	18	58	49		10		3	19	0	49		
	10		8	19	0	51½		11		4	19	4	50		
	11		15	19	4	51½			0	A	12	19	7	51	
		0	A	10	19	6	51½		1		5	19	5	51½	
		1		1	19	8	53		1		35	19	9	51½	
		1		40	19	8	54		2		0	19	8	52	
		2		10	19	7	54		3		0	19	2	51	
		3		1	19	5	53		8		0	18	57	49	
	11		40	19	0	47		15	0	M	14	18	57	45	
		8	M	10	18	55	47½		9		10	18	56	47	
12			10	18	56	49		10		11	18	56	47½		
	10		3	18	58	50½		11		10	18	58	50		
	11		2	19	3	51			0	A	3	19	1	50	
		0	A	11	19	6	52½		1		25	19	5	52	
	1		25	19	7	54		2		0	19	6	51½		

April.						April.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
15	5	A	12	19	1 50	19	1	A	40	19	7 51
	7		8	18	58 49		9		20	19	0 48
	9		55	18	58 44	20	0	M	17	19	0 47½
16	0	M	48	18	57 41		9		15	18	56 54
	9		5	18	58 41½	11			5	19	0 56
	10		0	19	0 42½		0	A	54	19	3 59
	11		10	19	5 44		1		38	19	4 59½
	0	A	5	19	6 45½		2		5	19	2 60½
	1		6	19	6 48		8		6	18	58 52
	2		0	19	6 48½	11			45	18	57 50½
	2		59	19	5 47	21	8	M	20	18	52 54
	4		0	19	2 47		9		10	18	54 55½
	5		5	19	2 46	10			5	18	56 56½
	10		2	18	32 42	11			20	18	59 59
17	1	M	20	18	55 40		0	A	10	19	4 62
	9		50	18	58 50		1		2	19	6 62
	11		1	19	2 50		1		42	19	7 63
	0	A	10	19	5 53		9		3	18	59 53
	1		30	19	8 53	11			50	18	58 49½
	6		0	19	2 49	22	8	M	35	18	55 55½
	9		8	18	58 43		9		15	18	55 56
18	0	M	35	18	59 41	11			5	18	56 60½
	8		30	18	56 46		0	A	2	19	1 60
	10		10	18	58 48½		1		16	19	4 63
	11		17	19	3 50½		2		10	19	4 62½
	1	A	0	19	7 50½		9		25	19	0 54
	1		40	19	8 50½	23	0	M	26	18	59 51
	2		15	19	7 50½		8		10	18	51 52
	3		10	19	5 49½		9		4	18	53 53½
	9		35	19	1 41	10			55	18	58 57
19	0	M	25	19	0 38		1	A	12	19	4 59½
	8		50	18	54 44½		1		40	19	4 60
	11		5	19	1 49		3		5	19	3 61
	1	A	0	19	7 50½		7		8	18	59 51

April.

April.						April.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
24	0	M	4	18 59	42	27	7	A	12	18 58	53
	7		45	18 55	45	28	1	M	10	18 57	49
	9		10	18 57	47		8		0	18 50	50½
	10		55	19 2	49		9		15	18 53	51½
	1	A	25	19 6	52		10		56	19 2	53
	3		15	19 5	51		0	A	2	19 6	55
	7		8	18 55	44		1		8	19 6	55
	9		3	18 52	41		1		40	19 7	55½
25	0	M	8	18 46	40		2		20	19 5	56
	8		45	18 57	48		3		20	19 3	55
	9		20	19 3	52		9		15	18 59	52
	11		8	19 6	55	29	1	M	2	18 59	51
	0	A	55	19 8	56		9		0	18 54	54½
	1		40	19 9	56		11		2	18 58	58½
	2		15	19 8	56		0	A	55	19 4	60
	3		20	19 5	55		1		32	19 3	60½
	5		12	18 59	54		5		4	18 59	57
	8		54	18 58	48½		9		26	18 59	47
26	0	M	10	18 57	46	30	11		42	19 1	44
	8		0	18 54	50		8	M	0	18 55	51½
	9		20	19 0	53		8		58	18 56	54
	11		10	19 6	56		11		6	19 3	61
	0	A	45	19 9	57½		1	A	2	19 6	63½
	1		25	19 10	58		1		50	19 6	63½
	2		0	19 8	56		2		52	19 4	62
	4		55	19 0	57		4		7	19 2	60½
	8		55	18 59	52		5		15	19 0	58
	11		42	18 58	50		7		55	19 0	51
27	8	M	5	18 50	55	May.					
	9		7	18 52	56	D.	H.	M.	°	'	Th.
	11		4	19 0	60	1	8	M	40	18 56	51
	1	A	6	19 5	60		10		56	19 0	57½
	2		8	19 4	59						
	3		12	19 2	58						

May.

May.						May.							
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.		
1	1	A	30	19	5	60	5	2	A	50	19	7	56
	4		3	19	2	57½		5		0	19	2	54
	9		2	18	59	48½	6	0	M	25	19	1	46
	11		55	18	58	46		9		18	18	56	54
	2	7	M	48	18	54	51	11		1	19	1	56
9			7	18	54	55	0	A	10	19	3	58	
11			7	18	59	58	0		55	19	4	58½	
1		A	28	19	3	59	1		35	19	4	60	
2			13	19	2	58	7	7		15	18	59	55
5		1	18	59	54	1		M	5	19	2	48	
8		0	18	58	48	8			10	18	49	54½	
0	M	15	18	58	46	9			10	18	54	57	
8		7	18	52	53	11			5	19	3	60	
3	9		30	18	53	54½	0	A	15	19	6	63	
	0	A	59	19	0	55½	1		2	19	8	63	
	1		47	19	3	55½	1		35	19	7	64	
	2		45	19	2	55½	3		4	19	4	62½	
	4		10	19	0	56	5		0	19	0	61	
4	11		58	18	59	51½	7		14	18	58	58½	
	8	M	15	18	51	56	10		0	18	58	56	
	9		16	18	52	58	8	0	M	20	19	1	54
	11		2	19	4	64		8		15	18	54	56
	0	A	20	19	6	64		9		8	18	57	57
1		5	19	7	64½	11			2	19	2	60½	
1		55	19	7	63	1		A	4	19	4	61½	
5	3		0	19	8	61½	3		10	19	1	63½	
	4		58	19	3	57	9	9		20	18	59	57
	9		30	18	39	53		0	M	2	18	58	55
	0	M	21	18	46	53		8		30	18	58	59
	8		25	18	56	52		9		20	18	54	60½
9		35	18	59	53	10			18	18	58	63	
6	11		0	19	2	55	11		35	19	3	64½	
	0	A	55	19	8	54	0	A	18	19	8	63½	
	1		40	19	9	55½	1		30	19	7	65	

May.

May.						May.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
9	3 A	8	19	3	64	12	5 A	0	18	59	64
	5	2	18	58	61		9	40	18	58	58
	9	5	18	58	55	13	0 M	3	18	58	57
10	0 M	25	18	56	53		8	30	18	53	60½
	8	5	19	3	55	11	2	18	59	64	
	9	20	18	59	59		0 A	45	19	3	64½
	11	15	19	2	59		1	30	19	12	64
	0 A	50	19	4	61½		2	13	19	8	64½
	1	15	19	6	62		5	12	19	1	62
	1	40	19	6	62		9	58	18	55	57
	3	20	19	4	61½	14	0 M	52	18	56	55½
	9	25	18	56	52½		8	10	18	54	58
11	0 M	15	18	52	51		9	12	18	56	58½
	8	5	18	52	55	11	13	19	0	62	
	9	25	18	53	57		1 A	15	19	4	65
	11	20	18	55	58½		2	7	19	5	64
	1 A	10	18	54	58		3	12	19	3	63
An Aurora Bor.	1	27	19	0	57½		7	13	19	0	57½
	1	37	19	2	57½		9	10	18	59	55
	2	15	18	47	57½	15	11	45	19	0	54
	3	0	18	58	59½		8 M	0	18	55	57½
	6	3	18	56	58		9	5	18	54	58½
	9	7	19	8	55	11	10	19	2	62	
	9	8	18	58	55		0 A	30	19	8	65
	9	9½	18	48	55		1	20	19	9	66
	9	28½	18	53	55		3	15	19	7	66
	9	32½	18	46	55		7	10	19	0	61
12	0 M	50	18	51	52	16	0 M	30	19	1	54
	7	55	18	51	57		8	10	18	53	60
	9	20	18	53	59		9	15	18	53	64
	11	15	19	1	62	11	28	18	59	69½	
	0 A	40	19	2	66		1 A	15	19	6	72
	1	12	19	3	65		2	0	19	5	72½
	3	0	19	3	65½		3	20	19	0	72½

May.						May.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
16	7	A	10	18	58 65	21	9	M	15	18	57 56
	8		40	18	58 62	11		0	19	1	61½
17	0	M	50	18	57 56	0	A	40	19	5	65
	8		20	18	53 56	1		24	19	7	66
	11			19	2 61	1		55	19	6	66
	0	A	4	19	8 62	6		3	19	1	65
	2		3	19	6 63	9		15	19	1	58
			4	18	59 52	22	0	M	5	18	56 54
18		M		18	57 49	8		15	18	52 56	
	8		1	18	56 52½	i	9		45	18	56 59
	9		15	18	57 53	11		2	19	2	59
	11			19	56	1	A	0	19	7	62
	1	A	20	19	57	1		30	19	6	61½
	2		10	19	57½	3		25	19	2	62
	6		50	19	52	5		35	18	58 61	
	9		0	19	47	9		20	19	0	54½
19	1	M	0	19	41½	23	0	M	25	19	0 52
	8		0	18	5 50				10	18	52 57½
	9		10	19	0 52	11		15	19	2	66
	11		10	19	6 55	0	A	5	19	6	67
	0	A	50	19	58	0		55	19	7	68
	1		40	19	58½	1		30	19	7	68
	2		20	19	6 59	2		5	19	8	67½
	3		2	9	4 60	6		55	18	57 58½	
	5		20	9	1 54	9		15	18	58 54	
20	0	M	10	9	0 44	11		50	18	56 54½	
	9		22	8	58 55	24	8	M	48	18	55 56
	10		26	9	2 7	11		5	19	7	57½
	0	A	30	9	1½	0	A	13	19	9	60
	1		40	19	6 1½	1		7	19	11	63
	2		35	19	6 12	1		48	19	9	64
	9		15	19	2	3		6	19	7	66
21	1	M	20	19	3 3	9		2	19	0	60½
	8		15	18	56 53	25	0	M	40	18	58 54

May.

May.

May.						May.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
25	8	M 20	18	56	58	29	9	A 30	18	56	47
	9	32	19	0	63		11	58	18	55	41
	11	15	19	8	70	30	8	M 20	18	56	49
	0	A 27	19	10	72		11	7	18	59	53
	1	20	19	9	72 $\frac{1}{2}$		3	A 5	19	3	56
	3	10	19	6	73 $\frac{1}{2}$		9	15	19	1	49 $\frac{1}{2}$
26	0	M 15	18	59	56		11	50	18	57	46
	8	5	18	54	60	31	8	M 45	18	54	54
	9	10	18	58	61 $\frac{1}{2}$		9	30	18	53	59
	10	15	19	2	63		3	A 55	19	0	63 $\frac{1}{2}$
	12	0	19	8	64		9	5	18	55	56
	0	A 25	19	9	64 $\frac{1}{2}$	June.					
	5	15	19	4	57	D.	H.	M.	°	'	Th.
	6	50	19	1	53	1	0	M 23	18	56	55
	9	40	19	0	46		9	5	18	51	59 $\frac{1}{2}$
27	0	M 40	19	2	43		11	0	19	0	61 $\frac{1}{2}$
	7	55	18	53	50		0	A 20	19	1	62 $\frac{1}{2}$
	11	A 58	19	3	51		1	10	19	1	63
28	8	M 55	18	54	57 $\frac{1}{2}$		2	15	19	0	64
	11	10	19	1	61		9	30	18	59	54
	0	A 35	19	6	63 $\frac{1}{2}$	2	0	M 15	18	56	52
	1	17	19	7	63		8	35	18	52	57
	4	25	19	3	63		11	12	19	0	63
	9	12	19	1	55		11	45	19	3	63 $\frac{1}{2}$
29	0	M 15	19	0	52		9	A 20	18	55	55
	8	28	18	56	57 $\frac{1}{2}$	3	0	M 40	18	56	54 $\frac{1}{2}$
	9	30	18	58	60 $\frac{1}{2}$		9	0	18	53	57
	10	25	19	0	60		11	14	18	58	56
	11	0	19	3	59		0	A 58	19	2	59 $\frac{1}{2}$
	0	A 15	19	7	59		1	30	19	3	59
	1	15	19	9	57		3	2	19	1	60
	2	15	19	7	53		5	30	18	58	58
	3	10	19	5	53						
	5	0	19	3	53 $\frac{1}{2}$						

June.						June.							
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.		
3	10	A	20	18	55	54	7	1	A	15	19	10	63½
4	0	M	50	18	54	53	11		50	18	59	53½	
	9		15	18	51	57½	8	7	M	6	18	53	53
	11		15	18	56	58½		9		10	18	57	60
	1	A	30	19	1	58½		11		30	19	3	63
	2		20	18	57	58	9	0	M	15	19	0	54
	3		12	19	0	57½		8		50	18	54	60½
	4		15	19	2	58½		11		48	19	6	65
	4		45	19	4	58		1	A	0	19	8	66
	7		30	19	0	54		1		35	19	9	66½
	9		30	19	1	50		3		10	19	6	66½
5	0	M	13	19	0	48½		5		0	19	3	64
	8		50	18	54	58		9		10	19	1	56½
	0	A	15	19	4	57	10	0	M	30	19	1	52½
	1		0	19	7	57		9		0	18	54	68
	1		47	19	8	58½		11		1	19	0	71½
	9		0	19	2	54½		1	A	12	19	6	72½
6	0	M	15	19	1	53		1		37	19	8	73½
	8		50	18	56	57		2		1	19	7	73½
	10		12	18	59	56		5		30	19	1	66
	11		15	19	2	57		7		15	19	0	63
	0	A	10	19	4	57		10		0	19	0	58
	1		12	19	7	58	11	0	M	52	19	0	57
	1		35	19	8	58		8		5	18	53	59
	3		0	19		60		9		4	18	53	61
	5		20	19	1	60		10		2	18	56	62
	7		15	18	58	57½		11		20	18	59	65
	8		30	19	0	56		1	A	2	19	6	65
7	0	M	2	19	0	53		1		32	19	7	65½
	8		30	18	52	58		3		30	19	5	59
	9		0	18	53	59		9		2	19	1	53
	10		1	18	58	60½	12	0	M	25	19	1	53
	11		12	19	5	62		7		48	18	50	56
	0	A	45	19	10	63		9		5	18	54	57

June.

June.						June.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
12	11	M	10	19	3 60 $\frac{1}{2}$	16	11	M	8	19	0 65 $\frac{1}{2}$
	1	A	2	19	8 62 $\frac{1}{2}$		4	A	55	19	0 6-
	5		15	19	2 60		9		2	18	59 59
	9		7	19	0 54 $\frac{1}{2}$	17	0	M	40	18	57 55
13	8	M	6	18	57 59 $\frac{1}{2}$		3		9	18	55 62
	9		10	18	57 61		9		0	18	56 62
	11		35	19	6 62 $\frac{1}{2}$	18	10		24	18	58 67
	1	A	15	19	8 61 $\frac{1}{2}$		11		15	19	2 63 $\frac{1}{2}$
	1		50	19	7 62		0	A	13	19	3 69
	3		0	19	6 62 $\frac{1}{2}$		1		0	19	5 69 $\frac{1}{2}$
	9		33	19	2 53 $\frac{1}{2}$		1		30	19	7 70 $\frac{1}{2}$
14	0	M	15	19	3 52		5		3	19	3 71 $\frac{1}{2}$
	8		7	18	56 59		7		0	18	57 68
	11		20	19	2 65 $\frac{1}{2}$	18	9		27	18	59 64 $\frac{1}{2}$
	1	A	0	19	6 67		0	M	45	19	0 62
	1		33	19	7 66		7		42	18	50 65 $\frac{1}{2}$
	3		10	19	5 67		8		52	18	51 70
	9		20	18	58 56 $\frac{1}{2}$		10		7	18	56 73
15	0	M	1	18	59 54		11		0	19	1 75
	7		45	18	58 59		12		0	19	4 75 $\frac{1}{2}$
	8		56	18	56 62 $\frac{1}{2}$		1	A	2	19	4 77
	10		15	18	59 64		1		40	19	5 76
	11		20	19	4 67		2		55	19	4 76 $\frac{1}{2}$
	0	A	25	19	6 65		4		7	19	1 75
	1		0	19	7 65		5		35	19	0 74
	1		30	19	8 63 $\frac{1}{2}$		7		20	18	59 71
	2		3	19	8 64		9		35	18	55 68
	3		7	19	5 62	19	0	M	15	18	56 64
	5		10	19	2 60		7		47	18	58 68
	7		30	18	59 59		9		5	18	58 72 $\frac{1}{2}$
	9		28	18	56 58 $\frac{1}{2}$		10		4	18	56 75
16	0	M	44	18	57 58 $\frac{1}{2}$		11		20	19	0 77
	7		40	18	55 61 $\frac{1}{2}$		0	A	20	19	5 79
	8		50	18	58 63 $\frac{1}{2}$		1		14	19	4 78

June.						June.							
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.		
19	3	A	20	19	3	78 $\frac{1}{2}$	22	9	A	12	19	2	67
	5		10	19	0	76	23	8	M	0	18	56	65
	6		8	18	57	74		9		15	18	58	68 $\frac{1}{2}$
	7		10	18	57	71 $\frac{1}{2}$	11		10	19	3	68	
	9		38	18	58	64	1	A	3	19	8	67 $\frac{1}{2}$	
20	0	M	18	18	58	60	1		40	19	7	67 $\frac{1}{2}$	
	8		20	18	57	68 $\frac{1}{2}$	3		16	19	5	67	
	9		10	18	57	70	5		35	19	0	66	
	11		5	19	1	75 $\frac{1}{2}$	9		14	19	1	62	
	0	A	40	19	6	76 $\frac{1}{2}$	24	0	M	32	19	1	58
21	4		2	19	4	74	8		0	18	56	63	
	7		1	18	57	69 $\frac{1}{2}$	9		20	18	57	65 $\frac{1}{2}$	
	9		3	18	57	64	11		6	19	3	69	
	0	M	25	18	56	61	1	A	5	19	7	71	
	8		0	18	56	71	1		48	19	8	70	
22	9		10	19	2	73	5		20	19	3	68	
	10		6	19	4	73	9		0	19	1	61 $\frac{1}{2}$	
	10		45	19	5	74	25	0	M	50	19	1	56
	11		16	19	6	75 $\frac{1}{2}$	8		0	18	56	62	
	0	A	2	19	5	77	8		55	19	1	63	
23	0		32	19	6	77 $\frac{1}{2}$	11		20	19	11	68	
	1		7	19	7	78 $\frac{1}{2}$	0	A	40	19	14	68 $\frac{1}{2}$	
	1		31	19	8	78 $\frac{1}{2}$	1		15	19	13	70	
	2		5	19	7	78	2		56	19	9	70 $\frac{1}{2}$	
	2		32	19	6	79	5		0	19	1	68	
24	3		5	19	5	77	7		16	19	1	64	
	3		35	19	4	76 $\frac{1}{2}$	9		30	19	2	63	
	4		21	19	1	75	26	0	M	1	19	2	61
	0	M	31	19	1	61 $\frac{1}{2}$	8		0	18	53	65	
	8		0	18	56	64 $\frac{1}{2}$	8		45	18	55	65 $\frac{1}{2}$	
25	8		50	19	0	65 $\frac{1}{2}$	10		57	19	5	69	
	10		10	19	3	70	11		50	19	9	72	
	11		5	19	5	71	0	A	31	19	11	72 $\frac{1}{2}$	
	1	A	25	19	8	74	1		10	19	11	72	

June.

June.					
D.	H.	M.	°	'	Th.
26	7	A	0	19	1 68
27	0	M	18	19	2 62
	6		4	18	58 62
	8		30	18	55 65
	9		2	18	54 67
	10		20	18	57 69
	11		40	19	4 68½
	0	A	50	19	9 70
	1		38	19	8 70
	3		10	19	8 68
	7		20	18	59 61
	9		12	19	6 59
	11		40	18	51 57½
28	0	M	10	18	50 57
	8		14	18	56 61
	9		1	18	57 63
	11		1	19	1 67
	0	A	35	19	7 68
	1		15	19	10 67½
	2		15	19	10 68½
29	0	M	14	19	0 57
	7		55	19	1 60
	8		58	18	59 63
	11		0	19	5 67
	0	A	35	19	9 69½
	1		15	19	11 67
	2		12	19	12 67½
	5		4	19	4 64½
	7		10	19	0 61
30	0	M	20	19	0 56
	8		3	18	56 58
	9		30	18	55 62
	11		5	19	0 64
	3	A	55	19	4 62½

June.					
D	H.	M.	°	'	Th.
30	9	A	0	19	0 58
	9		45	19	0 58

July.					
D.	H.	M.	°	'	Th.
1	0	M	25	19	0 57½
	9		5	18	52 60½
	11		4	18	57 60
	0	A	12	19	0 61
	1		0	19	3 61
	1		35	19	3 60½
	3		0	19	2 60
	5		5	19	1 58
	7		0	19	0 56½
2	7	M	58	18	54 57
	8		50	18	53 59
	11		15	18	57 63
	1	A	15	19	1 66
	1		52	19	2 67
	3		5	19	3 65½
	5		4	19	1 63
	9		15	19	0 59
3	0	M	8	18	49 57
	8		20	18	45 60
	9		15	18	53 62
	11		0	19	1 67
	1	A	3	19	5 71
	3		0	19	4 75
	7		30	18	59 67
4	2	M	0	18	56 56
	8		25	18	54 63
	9		10	18	57 64
	11		0	19	0 66

July.

July.						July.								
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.			
4	1	A	25	19	4	69½	9	9	M	15	18	59	73	
	2		20	19	6	71		11		20	19	1	76½	
	3		0	19	6	70½			0	A	40	19	7	78
	5		5	18	52	68			1		20	19	6	78
	7		45	19	0	63			2		10	19	12	79
	9		10	19	0	59			6		50	19	0	73
	1	M	3	19	2	54			9		20	18	49	68
	8		0	18	54	62			9		40	18	53	67
	10		57	19	1	68		10	8	M	0	18	53	72
	0	A	58	19	6	71			9		15	18	54	78½
5	2		10	19	5	72		11		5	18	59	82½	
	4		7	18	57	72		7	A	3	19	1	73	
	11		30	18	59	62		9		25	19	0	68	
	8	M	20	18	56	68	11	8	M	12	18	54	68	
	9		25	18	59	71½		11		20	19	2	72	
	11		20	19	4	75½		1	A	0	19	7	72	
	1	A	15	19	2	78		1		45	19	8	72½	
	2		5	19	2	78½		3		20	19	7	71½	
	3		20	19	3	78½		9		22	19	2	61	
	5		15	19	0	77	12	0	M	15	19	3	58	
6	9		12	18	58	70		7		55	19	0	66	
	0	M	32	18	58	67		9		10	19	1	69	
	8		17	18	50	73		0	A	20	19	11	70	
	9		18	18	52	76		9		5	19	3	65	
	11		20	18	57	79	13	8	M	20	18	53	68½	
	1	A	30	19	2	81		11		12	19	2	73	
	3		0	19	0	80		1	A	23	19	9	74½	
	0	M	5	18	57	64		2		0	19	10	76½	
	9		10	18	54	77		9		10	18	58	66	
	0	A	58	19	4	81	14	0	M	30	19	0	60½	
7	1		54	19	5	80½		8		25	18	55	67	
	5		0	19	4	74		9		10	18	58	68½	
	9		53	19	0	67		11		30	19	7	72	
	8	M	0	18	58	69		1	A	20	19	8	74	
	8													

July.

July.						July.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
14	2	A	30	19	7 74		11	A	40	19	1 63
	5		0	19	4 73		8	M	0	18 57	67½
	9		5	19	2 67		9		18	19	0 70
15	0	M	15	18	57 61		11		15	19	3 74
	9		5	18	58 70		1	A	35	19	7 73
	11		12	19	4 73		3		30	19	4 72
	1	A	3	19	9 74½		7		0	19	1 68½
	1		45	19	8 75		11		50	19	3 61½
	5		0	19	3 71	21	8	M	5	18 57	66
	7		15	19	2 67		11		20	19	5 72½
16	0	M	30	19	2 62		1	A	20	19	8 73½
	7		53	18	57 68		2		15	19	9 74
	9		55	19	0 67	22	0	M	20	19	2 59½
	11		30	19	4 73		9		0	18 57	69
	1	A	4	19	7 75		10		58	19	2 74
	2		5	19	8 75½		5	A	0	19	3 75
	6		50	19	3 68	23	8	M	0	18 59	70
	9		15	19	5 65		9		5	19	0 72½
17	0	M	8	19	4 63½		11		5	19	4 77
	8		20	19	1 66		1	A	5	19	8 78
	11		18	19	8 71		1		53	19	9 78½
	4	A	10	19	9 72		3		15	19	4 78½
	7		0	19	1 66		5		16	19	0 78½
18	0	M	10	19	0 60		9		30	19	1 65
	8		0	18 58	64	24	0	M	10	19	1 61½
	9		7	18 59	66		7		46	18 57	66
	11		15	19	4 72		9		15	19	0 69½
	1	A	35	19	7 73		11		44	19	7 72
	2		15	19	6 73½		1	A	15	19	9 75
	9		35	19	0 65		3		0	19	7 74½
19	7	M	45	18 57	65½		6		12	19	2 71½
	9		25	19	2 71	25	0	M	2	18 57	61
	11		30	19	8 74½		8		20	18 54	68½
	5	A	2	19	2 75		9		35	18 57	70

July.

July.						July							
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.		
25	11	M	10	19	4	74	29	4	A	40	19	11	72½
	1	A	20	19	11	77½	7	10	19	7	69		
	1		50	19	12	78	10	0	19	5	66		
	3		7	19	10	77	30	0	M	30	19	2	65
	9		30	19	1	67	8	0	19	4	65½		
26	11		50	19	2	62	8	50	19	1	68		
	8	M	0	18	58	66½	9	25	19	0	67½		
	9		17	19	1	71½	11	24	19	6	73		
	11		43	19	8	77½	1	A	15	19	9	74	
	1	A	17	19	11	78½	2	5	19	12	74½		
27	2		7	19	11	79	3	16	19	12	73		
	11		40	19	1	63	5	25	19	9	71½		
	8	M	15	18	55	66	6	54	19	6	68		
	9		12	18	57	66½	9	5	19	4	64		
	11		5	19	4	72	11	45	19	3	62		
28	1	A	2	19	12	72	31	7	M	58	18	57	63
	1		30	19	12	72	9	15	19	0	67		
	2		56	19	11	71	11	5	19	9	63		
	5		10	19	6	69½	1	A	12	19	15	62	
	3	M	20	18	59	55	1	35	19	16	62		
29	8		30	19	1	66	3	2	19	11	61½		
	9		25	19	1	67	4	45	19	8	60		
	11		3	19	6	70½	7	2	19	5	60½		
	1	A	2	19	12	73	9	23	19	5	58		
	2		5	19	14	72	August.						
29	3		40	19	12	74	D.	H.	M.	°	'	Th.	
	5		12	19	12	70½	1	0	M	7	19	4	57
	9		20	19	4	62	8	30	18	58	62		
	11		57	18	57	59½	11	12	19	6	66		
	8	M	25	18	57	66	2	A	7	19	11	68	
29	9		20	18	57	69	2	0	M	10	19	1	57
	11		10	19	1	72	8	25	19	0	60		
	1	A	35	19	11	73							
	2		40	19	13	74½							

August.						August.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
2	11	M	35	19	6	65	7	0	M	10	18 55 59
	1	A	26	19	11	68	7	40	18	58	60½
	9		38	19	4	56½	9	7	18	59	63
3	8	M	0	18	59	58½	11	20	19	4	66
	11		15	19	2	66	1	A	12	19	9 67½
	1	A	9	19	8	68	9	15	18	56	68
	2		10	19	9	68½	8	0	M	4	18 58 65
	5		20	19	0	67	8	15	18	58	68½
	7		15	18	59	63½	9	20	18	59	69½
	9		45	19	0	59	11	35	19	4	73
4	0	M	10	18	59	56	1	A	2	19	7 73½
	7		45	18	56	58½	2	55	19	2	77
	8		48	18	56	61	6	45	18	58	72
	11		2	19	3	61	9	0	M	32	18 57 63
	1	A	22	19	9	61	8	25	18	58	64
	2		40	19	6	61	11	12	19	6	75
	5		5	19	1	61	1	A	30	19	8 76
	7		6	19	1	61½	9	47	18	58	65
	9		30	19	1	62	10	8	M	12	18 57 70
5	0	M	12	19	2	61	9	15	18	57	74
	9		8	19	0	66½	11	0	19	2	79
	11		10	19	4	63	0	A	50	19	4 80½
	0	A	58	19	6	69	1	51	19	5	81
	4		37	19	3	63	3	32	19	2	79
	7		0	18	58	65	9	2	18	58	67
	9		2	18	59	63	11	0	M	30	18 57 63
6	7	M	32	18	55	58	8	15	18	53	68½
	9		11	18	56	62	9	13	18	56	73
	11		15	19	4	65½	11	2	19	0	77
	0	A	44	19	8	69½	1	A	17	19	4 81
	1		31	19	8	67	3	10	19	4	80½
	3		18	19	4	66½	6	15	18	58	77
	5		25	19	0	65½	12	0	M	12	18 59 67
	9		20	18	58	60½	8	25	18	58	62

August.					August.				
D.	H.	M.	°	Th.	D.	H.	M.	°	Th.
12	0	1	4	05	16	9	A	40	18 51 63
	0	1	9	8 55	10		12	13	49 62½
	1	0	19	9 65	11	0	M	10	13 54 60
	2	30	19	7 67		8		30	18 56 64
	7	0	19	0 64	11		28	19	7 68½
13	0	M	28	19 1 60		1	A	20	19 9 71
	7		55	18 58 62		4		0	19 6 69½
	9	20	19	1 65		6		0	19 9 67
	11	21	19	9 70		7		2	18 58 65
	1	A	25	19 9 71		8		45	19 5 61
	3	10	19	7 71	18	0	M	23	18 58 55
	7	13	19	2 64		8		15	18 58 58
	9	25	19	2 59		9		32	19 0 63
14	0	M	30	19 2 55	10		58	19	5 67
	8	10	18	58 61		1	A	4	19 7 69
	9	20	18	59 62		3		20	19 4 68
	1	A	0	19 8 67		5		4	19 3 66½
	7	2	19	2 64		6		52	18 59 65
15	0	M	38	19 1 61	19	0	M	40	18 59 59
	7		50	18 57 62½		9		15	18 58 63½
	9	30	18	59 65	10		58	19	3 67
	11	15	19	3 69		1	A	5	19 4 69
	1	A	17	19 6 70		1		52	19 4 68½
	2		0	19 5 67		5		55	18 59 67
	5	30	19	2 64½		7		15	19 0 65
	9	10	19	3 63	20	0	M	42	19 1 60
16	0	M	2	19 2 60		8		16	18 54 60½
	8		4	18 56 60½		9		25	18 57 64
	9	47	18	59 65	11		7	19	4 65½
	11	10	19	4 67		0	A	40	19 7 67
	1	A	4	19 11 69½		2		0	19 6 67
	1		32	19 10 70		5		10	19 0 65
	3		0	19 10 71		9		12	18 59 61
	5	12	19	4 69	21	8	M	20	18 53 59½

August.

August.						August.					
D.	H.	M.	°		Th.	D.	H.	M.	°		Th.
21	9	M	22	18	56	62	25	3	A	2	19 10 71½
	11		3	19	8	66½		9		30	19 3 60
	0	A	40	19	11	62	26	0	M	10	19 4 58
	3		32	19	2	65½		9		5	19 1 65
	5		20	18	59	63	11		10	19 5 70	
	6		2	18	58	62		1	A	0	19 12 72
	9		12	18	59	59		2		15	19 14 72
22	0	M	15	19	1	54		5		7	19 5 68½
	8		3	18	55	57½		7		16	19 4 64
	11		10	19	7	62½		8		55	18 54 61
	0	A	54	19	9	63	27	9	M	15	19 1 63
	3		15	19	5	64	11		12	19 8 68	
	6		53	18	59	60		0	A	40	19 13 69½
	9		20	19	0	56		1		30	19 14 70½
23	8	M	7	18	54	55		3		13	19 8 70½
	9		15	18	58	58		5		8	19 9 68½
	11		43	19	11	62		5		43	19 4 68
	1	A	0	19	14	66		7		15	19 4 64
	3		2	19	9	63		9		0	19 2 59½
	4		23	19	4	63½	28	0	M	5	19 7 56½
	5		54	19	3	62½		9		44	19 3 63
	9		15	19	4	58½		10		43	19 7 65
24	0	M	6	19	6	56		1	A	0	19 14 68
	8		20	19	3	59½		9		10	19 4 61
	10		8	19	9	63	29	10	M	25	19 6 65
	11		35	19	14	66½		1	A	30	19 13 67½
	1	A	9	19	24	67	30	0	M	25	19 4 51
	2		0	19	19	63		8		30	19 1 54
	4		55	19	8	62		1	A	15	19 15 62
	7		12	19	4	60		9		12	19 6 55
25	0	M	5	19	2	58	31	8	M	58	18 59 55½
	9		10	18	59	66	11		55	19 14 55	
	11		20	19	9	70		1	A	14	19 16 55½
	1	A	15	19	12	72		5		15	19 5 57

August.						September.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
31	8	A	32	19	5 52½	7	3	A	2	19	10 65
						8	1	M	46	19	4 57
							9		15	19	1 60
							11		6	19	6 64
							0	A	32	19	10 66
						9	0	M	46	19	4 58
							9		42	19	5 66
							11		35	19	11 72
							0	A	56	19	10 73
							6		51	19	4 69
							9		7	19	4 68
						10	0	M	40	18	37 66
							8		32	19	1 68½
							10		58	19	7 75
							0	A	56	19	18 77½
							1		15	19	20 78
							3		40	19	13 77½
							9		2	19	4 67
						11	0	M	20	19	4 63½
							8		22	19	4 62
							11		4	19	11 63½
							1	A	20	19	14 64½
							2		50	19	16 64
							5		25	19	7 62½
						12	0	M	23	18	54 55½
							8		30	19	8 59
							9		30	19	1 61½
							11		20	19	9 66
							1	A	25	19	13 67
							7		10	19	5 62
						13	0	M	54	19	4 61
							9		20	19	2 67
							11		45	19	11 69
							1	A	30	19	12 70

September.					
D.	H.	M.	°	'	Th.
1	9	M	33	18	59 55
	0	A	59	19	12 59
	9		5	19	3 57½
2	10	M	0	19	0 59½
	1	A	4	19	9 63
	2		50	19	12 62
	5		35	19	8 59
	6		43	19	1 58
	9		44	19	3 60
3	9	M	35	19	1 64
	1	A	58	19	12 68
	3		15	19	8 69
4	0	M	20	19	3 58
	9		3	19	4 59
	11		15	19	7 63
	1	A	14	19	11 65
	5		34	18	57 61
	8		40	19	1 56
5	0	M	13	19	6 54
	8		42	19	7 55
	11		2	19	9 59
	1	A	15	19	12 59½
	2		5	19	13 60½
6	8	M	45	18	59 60½
	11		56	19	9 65½
	1	A	15	19	10 67
7	0	M	30	19	5 58
	9		10	19	3 61½
	1	A	5	19	16 66

September.

September.						September.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
13	4	A 32	19	6	67	17	11	A 55	19	5	56
14	0	M 20	19	3	61	18	8	M 16	19	4	55
	8	15	19	1	58		11	25	19	15	61½
	9	25	19	3	60		6	A 0	19	5	57
	11	20	19	9	63		11	42	19	4	50½
	1	A 30	19	11	66	19	8	M 35	19	5	53
	3	40	19	10	63½		9	40	19	16	55
	5	32	19	7	61½		11	30	19	15	59
	11	55	19	2	54		1	A 32	19	19	60½
15	8	M 25	19	0	54½		3	2	19	12	60
	11	12	19	14	60		8	55	19	8	53
	1	A 30	19	13	61	20	0	M 20	19	2	48½
	8	55	18	53	54		8	40	19	1	50½
	11	58	18	53	53		11	20	19	9	58
16	9	M 30	19	9	58		1	A 40	19	1	60
	11	7	19	10	63		3	32	19	11	58½
	1	A 55	19	7	64½	21	0	M 2	19	2	48
	1	58	18	53	64½		8	45	19	1	56
	2	10	19	15	64½		11	10	19	9	60
	7	51	20	14	56		1	A 15	19	13	62
	7	52½	19	30	56		5	28	19	6	58½
	7	54½	19	14	56		9	16	19	1	54½
	7	57½	17	56	56		11	57	19	3	53
	7	59	17	44	56	22	8	M 28	19	0	56½
	8	5	18	34	56		11	4	19	5	63
	8	11	18	43	56		1	A 30	19	13	63½
	8	18	19	22	55½		3	18	19	12	63
	8	29	19	5	55		6	17	19	10	57
	9	25	18	55	53		9	5	19	4	52
17	8	M 15	19	1	56	23	9	M 55	19	5	59
	11	6	19	9	63		0	A 50	19	13	64
	1	A 17	19	13	64		7	22	19	6	67
	3	10	19	9	65	24	8	M 17	18	58	53
	7	0	19	4	61		11	30	19	9	62

A strong Aurora Borealis

September.

September.						September.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
24	1 A	35	19	16	65	29	9 A	7	19	4	51
	3	14	19	13	63	30	9 M	46	19	1	55
	8	54	18	53	56 $\frac{1}{2}$		0 A	5	19	14	59
	9	18	18	54	56		1	40	19	12	61
	11	52	19	18	53 $\frac{1}{2}$		11	44	19	4	48
25	8 M	25	18	59	53	October.					
	10	2	19	3	54 $\frac{1}{2}$	D.	H.	M.	°	'	Th.
	11	15	19	8	56 $\frac{1}{2}$	1	8 M	6	18	58	+7
	1 A	18	19	13	58		9	7	19	1	48
	5	16	19	8	53 $\frac{1}{2}$		2 A	0	19	14	62
	9	24	18	58	57		8	52	19	2	52
26	0 M	16	19	1	55		11	50	19	3	49 $\frac{1}{2}$
	8	46	19	0	57	2	8 M	20	19	14	54
	11	30	19	11	57		11	10	19	10	59 $\frac{1}{2}$
	1 A	7	19	12	59		1 A	12	19	14	60
	2	15	19	14	60		8	10	19	4	52
	3	12	19	15	58		9	21	18	49	49 $\frac{1}{2}$
	5	40	19	11	55		0 M	12	18	44	48
	11	45	19	14	49		8	41	19	9	48
27	8 M	43	19	1	53		10	2	19	8	50 $\frac{1}{2}$
	9	30	19	4	55		11	44	19	14	53
	1 A	0	19	12	59		1 A	35	19	15	54
	1	52	19	12	59 $\frac{1}{2}$		4	8	19	9	52 $\frac{1}{2}$
	9	16	19	5	53		8 M	35	19	0	54
28	8 M	17	19	18	60		11	14	19	9	58 $\frac{1}{2}$
	11	40	19	14	62		1 A	25	19	12	59
	1 A	27	19	15	63 $\frac{1}{2}$		9	11	19	3	53 $\frac{1}{2}$
	2	50	19	10	63		8 M	25	18	58	50 $\frac{1}{2}$
	5	20	19	5	59		9	46	19	0	56
	7	0	19	4	58		11	15	19	6	59 $\frac{1}{2}$
	0 M	5	19	5	55		1 A	12	19	10	62
29	9	15	19	1	59		2	30	19	8	62
	11	44	19	11	62						
	1 A	35	19	11	63						

October.

October.						October.					
D.	H.	M	°	'	Tn.	D.	H.	M	°	'	Tn.
5	9	A	18	19	6 58 $\frac{1}{2}$	12	11	22	19	7 59	
	11		16	19	4 58		1	A	20	19 12 60	
6	9	M	0	19	1 60		3		2	19 8 60	
	11		32	19	8 63		5	38	19	5 53	
	1	A	33	19	12 65		9		7	19 4 54	
	3		7	19	9 65 $\frac{1}{2}$	11		46	19	3 51	
	9		15	19	4 60	13	8	M	32	18 52 52	
7	0	M	22	19	5 56		11		30	19 7 57	
	10		1	19	1 63		1	A	28	19 14 58	
	1	A	23	19	9 67		9		44	19 3 50	
	9		10	19	4 57	14	10	M	7	19 3 56	
8	8	M	12	18	55 52		11		45	19 9 55	
	10		58	19	4 63		0	A	57	19 10 54	
	1	A	30	19	24 70		5		28	19 4 53 $\frac{1}{2}$	
	3		2	19	17 69 $\frac{1}{2}$		9		34	19 4 51	
	4		24	19	23 67 $\frac{1}{2}$	15	9	M	0	18 59 49	
9	0	M	28	18	26 56		10		52	19 7 53 $\frac{1}{2}$	
	8		26	19	23 54		1	A	13	19 10 57	
	11		15	19	14 67 $\frac{1}{2}$		3		15	19 8 57	
	4	A	21	19	7 68		9		18	19 4 56	
	9		40	18	54 60	16	0	M	32	19 5 55	
10	8	M	30	19	8 60 $\frac{1}{2}$		8		31	18 59 56	
	9		51	19	7 64		11		20	19 10 59	
	11		5	19	11 65		1	A	26	19 11 61	
	1	A	12	19	12 66		5		24	19 4 58	
	3		55	19	7 65		9		16	19 5 59	
	7		20	18	57 62	17	9	M	8	19 2 61	
11	8	M	7	19	3 58		11		16	19 11 61	
	11		13	19	7 60		1	A	28	19 10 59	
	1	A	32	19	10 61		3		27	19 6 57	
	3		15	19	9 59 $\frac{1}{2}$		6		24	19 5 53	
	5		30	19	5 58 $\frac{1}{2}$	18	10	M	13	19 4 50	
12	0	M	2	18	57 56		1	A	0	19 10 52	
	8		31	18	58 58 $\frac{1}{2}$		8		3	19 5 47 $\frac{1}{2}$	

October.

October.						October.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
19	8 M	41	19	1	42½	26	11 A	55	19	9	51½
	1 A	8	19	14	50	27	8 M	32	19	1	50
	3	12	19	13	49		1 A	17	19	14	47
	9	10	19	5	43		3	46	19	13	45½
20	9 M	32	19	2	39		9	2	19	7	41
	11	9	19	9	41½	28	9 M	46	19	4	42
	0 A	50	19	12	43		11	12	19	10	49½
	9	24	19	5	39½		1 A	3	19	12	52
21	3 A	7	19	9	53		5	10	19	9	49
	10	54	19	5	52		9	12	19	7	48
22	10 M	9	19	6	57	29	8 M	55	19	2	49
	0 A	44	19	11	59		11	0	19	8	52
	6	26	19	7	57		1 A	22	19	14	55
23	0 M	14	19	4	51½		3	35	19	12	54
	8	31	19	2	47		8	46	19	2	52
	11	32	19	9	55	30	8 M	40	19	6	50½
	1 A	4	19	10	56		11	8	19	15	52½
	5	8	19	8	52		1 A	6	19	17	53
	7	4	19	7	50		4	18	19	10	50
	11	41	19	7	45		9	32	19	4	44½
24	8 M	36	19	2	43	31	8 M	36	19	9	42
	1 A	8	19	14	53	A faint Aur Bor.	9	22	19	7	44
	3	10	19	12	51		1 A	22	19	18	50
	11	55	19	8	43		3	40	19	16	49
25	8 M	46	19	2	49		5	45	19	13	46
	11	9	19	9	53		9	32	18	54	42
	1 A	28	19	13	55	November.					
	2	57	19	11	54½	D.	H.	M.	°	'	Th.
	7	52	19	8	50	1	0 M	21	18	39	39
26	8 M	32	19	2	45		9	58	19	14	43
	11	20	19	10	48½		1 A	38	19	11	46½
	1 A	13	19	11	50		11	52	19	7	38
	3	0	19	9	51½						
	8	56	19	7	50½						

November.

November.						November.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
2	8 M	54	19	4	44	9	9 M	48	19	14	45
	1 A	26	19	10	50		1 A	37	19	18	50
	3	50	19	8	47½		9	35	19	3	52
	9	28	19	7	39½	10	8 M	35	19	7	46
3	8 M	40	19	4	37½		1 A	6	19	15	48½
	10	1	19	6	43		8	46	19	3	41
	11	24	19	10	47	11	10 M	7	19	7	42
	1 A	36	19	11	51		11	21	19	11	43½
	9	12	19	5	44		1 A	4	19	14	45
4	0 M	46	19	6	40		2	25	19	14	45
	10	2	19	6	48		7	28	19	10	40
	11	10	19	10	51	12	8 M	44	19	8	37½
	1 A	17	19	13	56		11	16	19	14	42
	5	17	19	9	55		1 A	6	19	15	44
5	0 M	25	19	6	55		3	20	19	12	42½
	9	36	19	3	57½		6	49	19	9	40
	1 A	28	19	17	58	13	8 M	36	19	8	40½
	6	54	19	4	54		11	29	19	15	43½
6	8 M	37	19	4	47½		1 A	7	19	16	46
	11	4	19	10	49		3	57	19	13	43
	1 A	7	19	15	51		9	58	19	9	38
	3	1	19	16	49	14	9 M	2	19	10	36
	5	16	19	12	45		1 A	48	19	15	43½
	9	17	19	10	44		3	4	19	14	42
7	8 M	14	19	9	46		7	46	19	9	37
	11	5	19	11	49	15	8 M	37	19	8	36
	1 A	26	19	14	52		10	55	19	12	41
	3	20	19	14	52		1 A	33	19	14	43
	7	17	19	10	47		4	36	19	10	40
8	0 M	46	19	11	42		11	30	19	9	33½
	8	52	19	11	42	16	8 M	47	19	5	31
	11	6	19	15	46½		11	47	19	14	38
	1 A	22	19	18	47		1 A	34	19	16	41½
9	0 M	31	19	12	44		3	36	19	14	40

November.

D.	H.	M.	°	'	Th.
16	5	A	45	19	9 37½
17	9	M	16	19	7 32
	11		9	19	13 37
	1	A	42	19	14 40
	3		26	19	13 38½
	9		5	19	7 33
19	9	M	7	19	7 32
	11		46	19	15 38
	1	A	15	19	15 38
	2		57	19	14 38
	6		3	19	10 35½
	9		38	19	7 34
20	0	M	13	19	9 35
	9		4	19	6 35½
	10		14	19	8 37
	11		35	19	10 38
	2	A	53	19	14 36
	5		2	19	12 35
21	0	M	7	19	9 34½
	9		5	19	5 37
	11		15	19	10 37
	1	A	12	19	14 37
	3		11	19	14 36½
	8		20	19	8 35½
22	0	M	10	19	6 34
	9		4	19	8 29
	11		28	19	12 34
	1	A	7	19	13 37
	3		12	19	13 36
	9		36	19	9 30
23	9	M	5	19	11 24
	11		12	19	16 26
	1	A	30	19	18 33
	3		37	19	15 33

November.

D.	H.	M.	°	'	Th.
23	8	A	52	19	6 31
24	0	M	14	19	3 31
	9		4	19	14 33½
	11		0	19	18 37
	1	A	16	19	20 38
	4		22	19	9 39
	7		23	19	6 39½
	9		57	19	5 39½
25	10	M	2	19	7 42
	11		40	19	10 46
	1	A	5	19	12 48
	4		22	19	9 49
26	8	M	58	19	8 53
	10		56	19	13 54
	1	A	23	19	16 54
	3		2	19	14 53½
	7		46	18	57 49
	9		25	18	44 48
27	8	M	42	19	2 42½
	11		12	19	26 46½
	1	A	0	19	19 48
	3		2	19	14 48
	5		33	19	14 47
	9		45	18	44 49
28	8	M	32	19	5 53
	0	A	7	19	13 53
	1		26	19	14 53
	2		52	19	11 51½
	6		0	19	7 47
	9		10	19	4 43
29	9	M	14	19	7 38½
	10		46	19	9 41
	1	A	20	19	10 44
	5		16	19	6 39

Faint Aur. Bor.

November.

November.						December.					
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.
30	0	M 25	19	4	33	7	1	A 35	19	8	37
	11	16	19	10	36		4	54	19	6	37
	8	A 53	19	4	32½		9	32	19	4	36
December.						8	0	M 25	19	5	35
							9	17	19	4	37½
						11	5		19	7	40
							1	A 4	19	11	42
							3	0	19	11	41
							5	37	19	9	37
							9	45	19	4	40
						9	0	M 16	19	5	39
							10	37	19	8	38
							1	A 21	19	10	41
							3	40	19	9	40
							11	48	19	7	39
						10	8	M 20	19	7	36
							10	24	19	9	38
							1	A 25	19	10	41
							3	48	19	9	40
							9	16	19	3	38½
						11	0	M 15	19	4	37½
							9	2	19	5	35
							11	10	19	9	36½
							0	A 47	19	10	37½
							3	28	19	12	37
							9	40	19	6	34
						12	9	M 14	19	7	30½
							11	3	19	9	34
							1	A 12	19	13	34½
							3	30	19	8	33
							9	5	19	6	30
						13	9	M 2	19	7	28½
							10	10	19	12	30
							2	A 1	19	14	31

December.						December.							
D.	H.	M.	°	'	Th.	D.	H.	M.	°	'	Th.		
13	8	A	58	19	4	30 $\frac{1}{2}$	20	10	M	32	19	9	37
14	9	M	5	19	7	31		1	A	58	19	12	40
	11		7	19	9	33		4		10	19	11	41
	0	A	55	19	14	33	21	9	M	5	19	8	37
	3		20	19	12	32		11		26	19	9	39
	6		17	19	1	31		1	A	0	19	11	39
	9		8	19	5	30		4		0	19	11	40
15	9	M	25	19	7	31		6		16	19	10	40
	11		24	19	13	35		9		15	19	9	39
	1	A	30	19	17	36	22	10	M	5	19	10	38
	3		20	19	8	33 $\frac{1}{2}$		11		56	19	12	39 $\frac{1}{2}$
16	0	M	15	19	7	29		1	A	49	19	16	40
	10		22	19	8	33		5		6	19	10	39
	1	A	16	19	13	36		8		50	19	9	38
	5		37	19	13	31 $\frac{1}{2}$	23	0	M	48	19	8	37
	9		15	19	7	33 $\frac{1}{2}$		10		30	19	16	38
17	9	M	0	19	6	26 $\frac{1}{2}$		5	A	2	19	9	39 $\frac{1}{2}$
	11		12	19	10	32 $\frac{1}{2}$		5		10	19	4	39 $\frac{1}{2}$
	1	A	40	19	11	34 $\frac{1}{2}$		5		17	19	14	39
	3		8	19	12	33 $\frac{1}{2}$		5		22	19	14	39
	9		25	19	8	26 $\frac{1}{2}$		6		30	19	1	39 $\frac{1}{2}$
	11		56	19	7	25		7		53	19	2	37
18	9	M	7	19	7	25		0	M	40	19	6	37
	11		10	19	11	31	24	10		25	19	10	36
	1	A	12	19	14	33		1	A	40	19	11	36
	5		23	19	12	32 $\frac{1}{2}$		3		25	19	9	36
	9		10	19	8	32		6		41	19	6	36
19	9	M	5	19	7	32 $\frac{1}{2}$		9		18	19	5	36
	11		30	19	13	33 $\frac{1}{2}$	25	9	M	34	19	6	40
	1	A	25	19	16	34		11		30	19	9	42
	5		37	19	9	34		2	A	0	19	10	43 $\frac{1}{2}$
	9		10	19	8	32		3		25	19	11	43
20	0	M	5	19	8	32 $\frac{1}{2}$		5		56	19	10	43
	9		7	19	8	36		8		30	19	6	43

December.

December.					December.				
D.	H.	M.	°	Th.	D.	H.	M.	°	Th.
26	9	M	40	19 6 $42\frac{1}{2}$	29	3	A	15	19 14 45
	11		0	19 7 46		5		42	19 14 $44\frac{1}{2}$
	1	A	30	19 13 47		9		5	19 5 45
	3		0	19 12 $46\frac{1}{2}$	30	0	M	12	19 8 46
	7		22	19 10 $42\frac{1}{2}$		10		25	19 10 47
	9		4	19 8 $42\frac{1}{2}$		11		35	19 11 47
27	0	M	2	19 7 43		1	A	3	19 12 47
	10		24	19 17 38		2		0	19 13 $46\frac{1}{2}$
	1	A	28	19 18 42		5		43	19 11 $46\frac{1}{2}$
	3		2	19 13 41		9		32	19 9 47
	4		47	19 9 $39\frac{1}{2}$	31	0	M	33	19 9 46
28	11	M	23	19 14 $45\frac{1}{2}$		9		18	19 8 43
	1	A	15	19 18 47		11		2	19 9 $45\frac{1}{2}$
	6		30	19 9 46		1	A	14	19 14 47
	9		11	19 8 44		2		57	19 13 46
29	10	M	0	19 17 46		5		30	19 10 44
	0	A	47	19 14 46		9		2	19 8 $42\frac{1}{2}$
	1		43	19 17 $45\frac{1}{2}$		11		57	19 8 42

The mean diurnal Variation for
each Month in the Year 1759.

	'	"
January - - -	7	8
February - - -	8	58
March - - -	11	17
April - - -	12	26
May - - -	13	0
June - - -	13	21
July - - -	13	14
August - - -	12	19
September - - -	11	43
October - - -	10	36
November - - -	8	9
December - - -	6	58

XXXIX. *A Letter to the Right Honourable Hugh Earl of Marchmont, F. R. S. concerning the Sections of a Solid, hitherto not considered by Geometers; from William Brakenridge, D. D. Rector of St. Michael Bassishaw London, and F. R. S.*

My Lord,

Read Dec. 20,
1759.

YOUR knowlege in Geometry, and the other Sciences that depend upon it, makes me presume to lay before you the following speculations. Your benevolence to all philosophical Inquiries encourages me, and the personal regard I have for your Lordship induces me to do myself this honour; and tho' what I offer at present may be of no great consequence, I am persuaded, that every little accession to our knowlege will give you some pleasure, as you very well know, that all our improvements in science are slow, and from small beginnings. You have here a new method of considering some geometrical curves, from the sections of a solid, hitherto not taken notice of, and by which, in particular, you will see, that the two infinite curve lines from the section of the cone, are also the sections of this; which may be of some use, as it seems to extend our views of their nature and properties. The description of it is very easy and obvious, and it has something remarkable in its form, that tho', in the most simple case, it is generated and bounded by right lines, the surface is incurvated. The solid is thus described.

Fig 1.

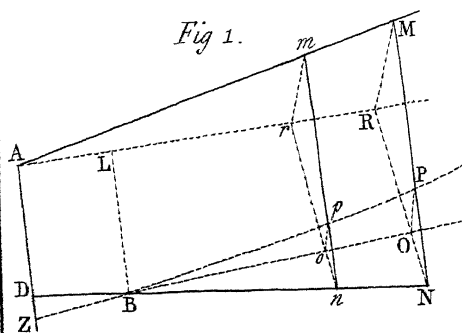


Fig. 2.

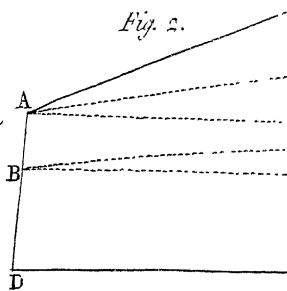


Fig. 4.

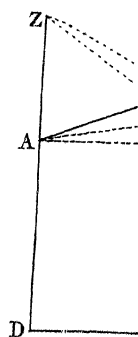
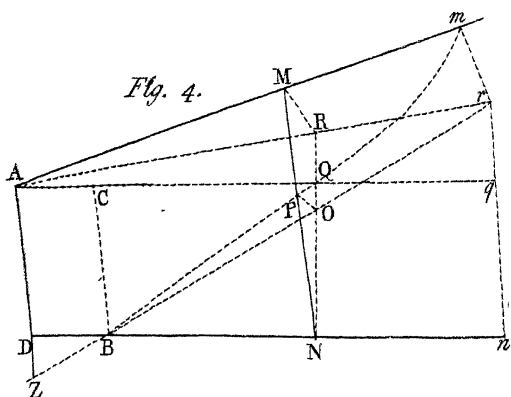


Fig. 8.

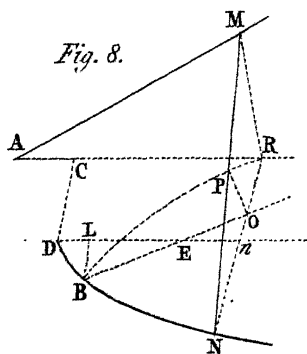
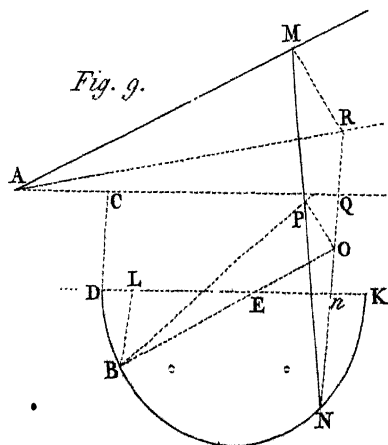
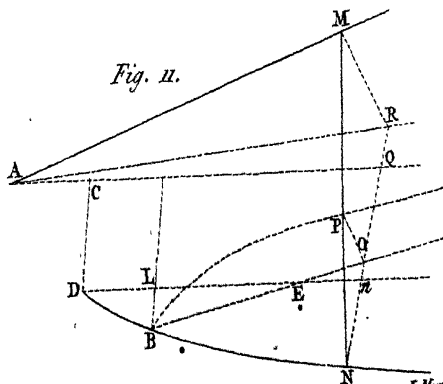
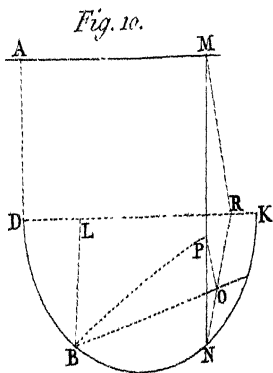
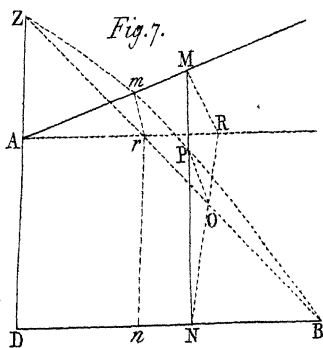
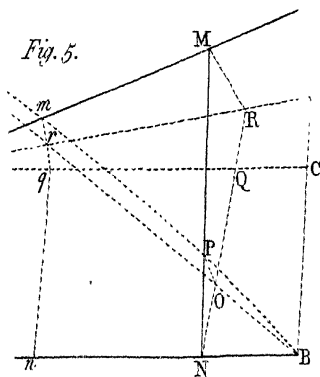
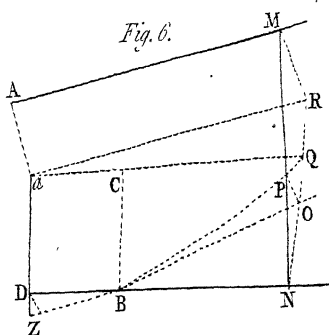
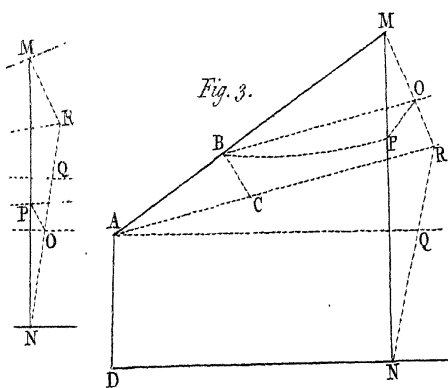


Fig. 9.





Let DN be a right line drawn on a plane, and a point given A at any distance from the line, from which having raised, above the plane, the right line AM in any given angle, and drawing from the same point A the line AD , meeting the line DN on the plane in D , make another plane MNR to move parallel to itself, and to that line AD in a given angle to the first plane; and then, suppose the intersections of it M, N, m, n , &c. with the lines AM and DN , to be continually joined with right lines MN, mn , &c. and there will be generated an incurvated surface by them, and bounded by the lines AM, MN, ND, AD . *Plate X. Fig. 1.*

The same surface will be described, if a line MN be supposed to move in such manner, along the lines AM and DN , that the parts AM and DN be continually in a given ratio. For let a plane be drawn thro' the raised line AM , perpendicular to the given plane ADN , and their common intersection be the line AR ; then having joined the points A and D , from the point N , while the line MN is moving, let there be continually drawn a line NR parallel to DA , meeting the line of intersection AR of the planes in R , and the proportion of DN to AR will always be given. But as the ratio of DN to AM is given, the ratio of AM to AR will also be given; and the line RM being drawn, the angle ARM will be given; and therefore the plane of the triangle MNR will move parallel to itself, and to the line AD , making the given angle MRA with the plane ADN , and the line MN will generate the same surface as before.

It is evident this incurvated surface may be infinitely extended on all sides beyond the lines AM , MN , ND , AD , and as well below the given plane ADN as above it; and therefore the various sections of it, if continued, will be infinite lines.

The line AM , raised above the given plane ADN , may be called the vertical Directrix.

And if thro' this directrix AM there be drawn a plane AMR in any given angle to the plane ADN , intersecting the moving plane NMR in the line MR , and meeting the given plane ADN in R ; then the points A , R , and R , N , being joined, there will be a trapezium $ARN D$, formed, that may be named the Base, and in which the line DN may be called the Directrix of the Base.

And the plane AMR passing thro' the vertical directrix and the moving plane MNR , together with the base $ARN D$, and the curve surface, will make a solid $AMRND A$, something in the form of a wedge.

In this solid there may be made five sections in a given angle with the base, or parallel to it; viz, one parallel to the moved plane; one parallel to the directrix of the base; another parallel to the vertical directrix; a fourth parallel to the base; and a fifth intersecting both the directrices. And in all these cases, when the directrices are right lines, the sections will be either the conic hyperbola, or the parabola, or right lines.

1st, If the solid $AMRND A$ be cut by a plane parallel to the moved or generating plane NMR , or the line AD , the section will be a right line. This is evident from the description, *Fig. 1.*

2. If the section be made by a plane B P O parallel to the directrix of the base D N, and in any given angle to the base, it will be an hyperbola, *Fig. 2.*

Let the section P B O meet the base in the line B O, which will be parallel to D N, and make the moving plane N M R intersect the base in N R, the vertical directrix in M, and the section in P O; by which D B = N O. From the point M draw M R parallel to P O, and imagine a plane to pass through the directrix A M and the line M R, meeting the base in A R; which will be given in position. Then thro' the point A draw A Q, parallel to D N, intersecting N R in Q, and making N Q = A D; and the two triangles A R Q, A M R, will have all their angles given, and the proportion of their sides. And therefore the ratio of A Q to Q R, and of A Q to M R, will be given. Make A D = a , B D = b , A Q : Q R :: $a : q$, and A Q : M R :: $a : m$, the abscisse B O = x , and the ordinate P O = y ; from which $Q R = \frac{q x}{a}$,

$N R = a + \frac{q x}{a}$, $M R = \frac{m x}{a}$. And then from the similar triangles N O P, N M R, the analogy N O : O P :: N R : M R will give the equation $y x q + y a^2 = x b m$, and the curve B P is an hyperbola.

3. If the section P B be made by a plane parallel to the vertical directrix A M, it will be an hyperbola. *Fig. 1.*

Let the moving plane M R N, parallel to D A, intersect the base in R N, and the vertical directrix in M, and make the plane of the section B P O cut the moving plane N M R in P O, and the base in the line B O, meeting D A in Z, and the base directrix D N in B; from the point M draw the line

MR parallel to the plane of the section, and meeting the base in R; and the lines MR and PO will be parallel. Then thro' the vertical AM, and the line MR, make a plane to pass, which will be parallel to the section, intersecting the base in AR, and the lines AR and BO will also be parallel. Draw BL parallel to DA, or NR, passing AR in L, and then BO=LR, BL=RO=AZ, and AL=BZ. And the line AR being given in position, the lines BL and AL will be given. The angles also MAR and NBO being given, the ratio of AR to RM, and of BO to ON, will be given. Suppose AD \propto BL=a, AL=c the abscisse BO=x, the ordinate PO=y, and AR:RM::a:m, and BO:ON::a:n, from which we have $ON = \frac{nx}{a}$, $RM = \frac{mc + mx}{a}$;

$NR = a + \frac{nx}{a}$. And then, in the triangles on the moving plane, which are similar, MRN, PON, the analogy NO:PO::NR:RM will produce the equation $y a^3 + n x a y = x^2 n m + x n m c$; which shews the curve BP to be an hyperbola; and the figure is convex to the base.

4. The section being made parallel to the base, it will be the same curve. *Fig. 3.*

Having thro' the vertical directrix made a plane AMR to pass perpendicular to the base, let the section BPO, and the base parallel to it, meet that plane in the lines BO and AR, and also the moving plane NMR in the lines PO and NR. From the point A draw AQ parallel to the directrix DN of the base, and AD parallel to NR, and from B the line BC parallel to OR, meeting AR in C; and then

then the lines AD , AC , and BC , will be given, and the angles RAQ , MAR , being also given, the proportion of AR to QR , and of BO to MO , will be known. Make $AD=a$, $AC=c$, the abscisse $BO=x$, the ordinate $PO=y$, and $AR:RQ::a:q$; from which we have $MO:MR::x:x+c$, $QR=\frac{cq+qx}{a}$. And because, in the similar triangles MPO , and MNR , in the moving plane, we have $MO:PO::MR:RN$, there will result the equation $yx a + y c a = x^2 q + x c q + x a^2$; which denotes the curve BP to be an hyperbola.

5. If the section is made so as to meet the two directrices, the curve will be also an hyperbola. *Fig. 4.*

Let the section BPO meet the directrices in B , m , and intersect the plane of the base in the line BO ; and make the moving plane NMR to be cut by the section in PO , and to meet the vertical directrix in M . Then from the point M draw MR parallel to PO ; and thro' the lines AM , MR , imagine, as before, a plane to pass, intersecting the base in the line ARr , and meeting the line BO in r , and the section in mr . And from A draw AD parallel to NR , and AQ to DN ; and thro' r make nr parallel to NR , and to meet AQ in q , and DN in n . Draw also from the point B the line BC parallel to AD , and meeting AQ in C . The lines then Aq , Ar , mr , rq , Br , Bn , rn , will be given. Make therefore $AD=a=BC$, $AQ:QR::a:q$, and $AQ:RM::a:m$, $Br:Bn::a:b$, $Br:rn::a:n$, $AC=DB=c$; the abscisse $BO=x$; the ordinate $OP=y$. From which we have $BN=\frac{bx}{a}$, $ON=\frac{nx}{a}$, $AQ=c+$

bx

$$\frac{bx}{a}, RQ = \frac{qca + qb^2x}{a^2}, MR = \frac{mca + mb^2x}{a^2}. \quad \text{And}$$

then, in the similar triangles NPO , MNR , having $NO:PO::NR:MR$, the equation will be $ya^4 + yca^2q + yxb^2aq = nmcax + nmb^2x^2$. And the curve is an hyperbola; and in the case of this *Fig. 4.* it will be convex to the plane of the base. But when BN is negative in the case of *Fig. 5.* the equation, retaining the same symbols, will be $ya^4 + yqca^2 - yxb^2q = mncax - nmb^2x^2$; and the hyperbolic curve will be concave towards the base.

6. If the vertical directrix AM is made parallel to the plan of the base, but the plane passing thro' it not parallel to the other directrix, then the section, meeting the two directrices, will also be the same curve. *Fig. 6.*

For in this case the line MR is a constant quantity; and therefore, if the common section aR of the plane thro' the vertical AM , with the base, meet aD parallel to NR in a , other being as before, making $MR=m$, and $Du=a$; from the analogy $NO:PO::NR:MR$ in the triangles NOP , MNR , we shall have $mna^2x = ya^3 + yacq + yxb^2$; by which the curve is known to be an hyperbola.

And in all those sections, where the common intersection of the plane, passing thro' the vertical with the base, is not parallel to the other directrix, the curve is an hyperbola.

7. But now, if we suppose the common intersection AR of the plane passing thro' the vertical, with the base, to be parallel to the directrix DN of the base, and both directrices to be cut by the section, the curve will be a parabola. *Fig. 7.*

For

For, in this case, the two lines AR and AQ coincide, and AR is parallel to the directrix DN of the base; and therefore, using the same symbols as above, the equation, from *Fig. 4.* will be reduced to $ya^4 = nmcax + nmbx^2$; and from *Fig. 5.* it will come to $ya^4 = nmcax - nmbx^2$; both which shew the curve to be a parabola.

It may also be demonstrated in this manner. It is evident, that $PO : mr :: PO \times MR : mr \times MR$, that is, the ratio of PO to mr is equal to that compounded of $PO : MR$, and of $MR : mr$. But from the similar triangles MRN, PON, we have $PO : MR :: NO : NR = nr$, and from the triangles BON, Brn, we have $BO : Br :: NO : nr$; therefore $PO : MR :: BO : Br$. In like manner, from the equiangular triangles MRA, mrA , there will be $MR : mr :: RA : rA$, and from the triangles RO r , ZAR, it is $RA : rA :: Oz : rz$; therefore $MR : mr :: Oz : rz$. If then in the ratio of $PO \times RM$ to $mr \times MR$, we substitute the ratio of $BO : Br$, and of $Oz : rz$, which are equal to $PO : MR$, and $MR : mr$, we shall have $PO : mr :: BO \times Oz : Br \times rz$; which is a known property of the parabola.

And thus I have endeavoured to extend, a little, the theory of the conic sections. I have here shewn how two of them may be had from the sections of this solid; and in the year 1733, I published, in my *Exercitatio Geometrica*, a method of describing all of them on a plane, by the moving of three right lines about three given points, two of the intersections being drawn along two other lines given in position; the only hint of which I had from a locus of an ellipsis, in the construction of a clock, by the celebrated M. De

la Hire, in the French *Memoires* for the year 1717, intituled, *Construſtion d'une Horologe qui marque le vrai tems avec le moyen.*

But now, having briefly conſidered the various ſections of this ſolid, the directrix DN of the baſe being a right line, let us next ſee what they are when it is a curve line of any order. And becauſe there are an infinite number of caſes, it will be ſufficient to mention a few of them, when that directrix is a conic ſection or circle, and then to give a general propoſition when it is any higher geometrical curve.

8. If the directrix DN of the baſe be a parabola, having a diameter parallel to the interſection AR of the plane, paſſing through the vertical A, and both directrices be cut by the ſection, the curve BP will be a line of the fourth order. *Fig. 8.*

Let the diameter be DLn, the ordinate Nn, and the equation of the parabola $u^2 = zp$, and make the moving plan MNR to paſs through the ordinate Nn, and the ſection to interſect the baſe in BO, and the diameter in E. From the points B and D draw BL and DC, parallel to NO, and meeting the lines AR and Dn in C and L. Then ſuppoſe AR : RM :: a : m, and BO : Ln :: a : n, AC = c, DC = d, BL = b, DL = l, BE = e, Nn = u, Dn = z, the abſciſſe BO = x, the ordinate PO = y; and we have

$$Ln = z - l = \frac{nx}{a}, \quad no = \frac{b \times x - e}{e}, \quad MR = \frac{mc + mz}{a} \\ = \frac{mca + mal + nmx}{a^2} \text{ and } u^2 = \frac{ap l + p x n}{a}. \text{ But from}$$

the ſimilar triangles NPO and NMR we have this proportion NO : PO :: NR : MR, which gives the

$$\text{equation } \frac{bx - be \times \frac{mca + mal + nmx}{a^2} - dy a^2 e}{y a^2 e - mca e - malle - nmx e} = u$$

$= \sqrt{\frac{a p l + p x n}{a}}$; and this being reduced, shews, that the section P B is a line of the fourth order. But if the section be made parallel to the vertical, the curve will be a line of the third order.

9. If the directrix D N be a circle, and other things being as before, the section will be a line of the fourth order. *Fig. 9.*

Make the center of the circle to be in the line D L *n* parallel to A Q, the ordinate to be N *n* = *u*, the abscisse D *n* = *z*, the radius = *r*, and the equation $u^2 = 2 r z - z^2$. And let the plane passing through the vertical, cut the base in A R, and the section meet the diameter D K in E. Then the same things being supposed, and the symbols retained as before, we shall have L *n* = *z* - *l* = $\frac{x n}{a}$, $z = \frac{a l + x n}{a}$,

$$n O = \frac{b x - e b}{e}, \quad Q R = \frac{q a c + q a l + q n x}{a^2}, \quad M R = \frac{m a c + m a l + m n x}{a^2}, \quad \sqrt{\frac{2 r l a^2 + 2 r x n a - x^2 n^2 - 2 a l n x - a^2 l^2}{a^2}}$$

= *u*. And the analogy N O : P O :: N R : M R will give this equation, $\frac{m a c + m a l + m n x}{a^2} \times \frac{b e - b x}{e}$

$$+ \frac{y a^2 d + y q a c + y q a l + y q n x}{a^2} = \frac{m a c + m a l + m n x - a^2 y}{a^2} \times \sqrt{\frac{2 r l a^2 + 2 r x n a - x^2 n^2 - 2 a l n x - a^2 l^2}{a^2}};$$

from which it appears, that the curve B P is a line of the fourth order.

And in general it may be seen, that if the directrix of the base be a conic section, except in the case above, the section of the solid will be a line of the fourth order.

10. If the vertical be parallel to the base, and the plane passing through it perpendicular, the directrix of the base being a circle, having its center in the intersection AR of the two planes; then the solid will be the cono-cuneus of the learned Dr. Wallis, and the curve sections of it will be also, some cases excepted, lines of the fourth order. *Fig. 10.*

For, in this case, the quantities QR and DC will vanish, and making MR = m , the equation, retaining the other symbols, will be $\frac{m - y}{a^2} \times \sqrt{2rxna + 2rla^2 - x^2 - 2xnl a - a^2 l^2} = m \times \frac{eb - xb}{e}$.

And here it is surprising that the great Doctor, while he was considering his solid, did not fall upon the one I have explained; but indeed, in searching after new discoveries, we are often like those, who, groping in the dark, miss the things that are nearest them.

11. To conclude, if the directrix DN of the base be a line of any order n , the section BP will be of the order $2n$. *Fig. 11.*

In the equation of the curve directrix DN of the base $u^n = A z^n + B z^{n-1} u + C z^{n-2} u^2 + D z^{n-3} u^3$, &c. make the abscisse Dn = z , and the ordinate Nn = u ; and draw AQ parallel to Dn, and then, other things being as before, the analogy NO : PO :: NR : MR will be thus expressed, $u + \frac{bx - eb}{e} : y :: u + d + \frac{qnx + qal + qac}{a^2} : \frac{nm x + mal + mca}{a}$; from which we have $u =$

$$\frac{eb - bx \times nm x + mca + mal}{emal + emnx + mace - a^2 ey} + \frac{da^2 y + nqxy + qaly + qyac}{mnx + mac - a^2 y + mal}$$

And because Dn = $z = l + \frac{nx}{a}$, if we substitute these values of u and z , in the above general equation, the

the line of the section BP will appear to be of the order 2 π .

And now, my Lord, I have given you some general propositions about the various sections of this solid, and I have shewn how lines of any order may arise from them; which is a new theory, and perhaps may introduce to something farther. I have other things of this sort, that relate to what I have published in the Philosophical Transactions, Vol. 39. in the year 1735, which I have had many years by me, that I intend to send you, if my health, and the circumstances of my life, will allow me to revise them. And in the mean time, with great respect, I am,

My Lord,

Your Lordship's most obedient
and affectionate servant,

Sion College, Dec. 18,
1759.

W^m. Brakenridge.

END of PART I.

I. A. R. I. 75.

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